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The Journal of Parapsychology

A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

Volume 9 MARCH, 1945 Number 1

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The Journal *of* Parapsychology

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The Journal *of* Parapsychology

Volume 9 MARCH, 1945 Number 1

EDITORIALS

PARAPSYCHOLOGY AND RELIGION[1]

THE SCIENCE of psychology bears a relation to the field of religion which is like that of physiology to medicine, and physics to engineering. But this is apparent only when we consider within the scope of psychology its newest acquisition, the psychology of paranormal (at present unexplainable) manifestations, which is called *parapsychology.*

The aim of religion is to provide, through its doctrine, answers to the most fundamental questions about the nature of man and what he can do to control his destiny. It does not alter this fact for us to recognize that religion offers, as yet, no answers that have been acquired by the methods of science. (There was a time when the same could be said of medicine; but the rise of physiology has changed all that.) The objective of parapsychology is to discover the nature of human personality by taking into account all reliable evidences of exceptional psychical phenomena that can be studied. If parapsychology finds answers to the questions for which religious doctrines have been developed in the past, there is no reason why these should not replace the earlier conceptions in much the same way that chemistry has replaced alchemy and scientific medicine has taken the place of the practices of prescientific days.

It is reasonable to hope, therefore, that the attitude of the true student of religion will more and more approximate that of the scien-

[1] This editorial is a digest of an article which appeared in the *Christian Register* for August, 1944.

tific inquirer. We can all appreciate the very great service religion has rendered in keeping these fundamental problems of man's nature alive through the ages, even though we question the finality of the solutions that have been offered; for, as we all know, it is the *questions* and not any given set of doctrinal answers to them that make religion so important.

The basic question for any religion is whether there is a spiritual factor in man. Without a positive answer to this question there would probably be nothing that could rightly be called a religion. The term "spiritual" is, of course, a religious rather than a scientific one, but its meaning is relatively clear and can be converted to scientific terminology. It means, first of all, "extraphysical"—something subject to principles that lie beyond the physical system of nature. Originally it may have denoted simply the immaterial, but as the term "physical" has itself expanded beyond the range of simple matter and its motion, the concept of the spiritual has become adjusted to mean "beyond the mechanistic or physical systems as we know them."

The findings of parapsychology have a bearing on this basic question of religion which appears to be unmistakable. They have yielded varied and extensive experimental evidence of extraphysical processes in human subjects; and this, if the foregoing definition of extraphysical is correct, means that they have confirmed the occurrence of a spiritual factor in human personality.

The first, though not necessarily the best, evidence of this nonphysical type of process in man came out of the experiments in *extrasensory perception* which established the fact that spatial conditions do not limit the process. From the common viewpoint, based upon sensory and mechanical analogies, this extraphysicality appears incredible, partakes of the nature of the miraculous, the unexplainable, and that is why so extensive an accumulation of evidence is demanded before acceptance. But to those who are familiar with the astonishing array of independent confirmation that now exists, the exclusion of a physical explanation of the ESP process has been accomplished and the supposition of a nonphysical order of causation has been made necessary.

Another type of evidence derives from the experiments on the hypothesis of *precognition*. Fragmentary and, on the whole, im-

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practical though this precognitive ability is from what we know of it thus far, the evidence that it does exist to a demonstrable degree is now good enough to establish the phenomenon as a reality. It may be very difficult to incorporate precognition in our body of organized knowledge, but for the student of religion it is another confirmation of the existence of a spiritual factor in man; for perhaps no more striking evidence of the extraphysical character of the mind could be furnished than the discovery that time (at least what the physicists call time) is no more a boundary or limitation to the mental world than, in the ESP experiments, space proved to be. After all, this is as it should be, for the world of physics is a space-time world, and a process free of space limitations should be expected to be in the same degree free from the boundaries of time.

Still a third convergence of parapsychological discovery upon the religious question which we are discussing comes from the recently published reports of *psychokinesis* or PK, the dominance of mind over matter. These experiments compare favorably with the ESP studies in the degree to which they focus the issue as to whether the mind operates on the basis of physical or extraphysical principles. The purposive character of the influence exerted on the dice is so obviously nonphysical as to leave no further debate.

When we think of the great problems of religion that yet remain untouched, the advances mentioned seem very modest indeed. We do not even know as yet how far we can generalize from the special capacities studied to the total realm of mental life. We cannot safely argue that because certain capacities of the mind are not limited by the space-time relationship of the physical world, the mind is therefore a separable and independent system which may have an existence of its own. The answers to this and other major research questions are still to be found. But what has been done has had to come first in order to provide an approach to the remaining questions.

This last sentence needs amplification. It was not possible to come to grips definitely with the question of immortality or personal survival of bodily death without some clearing-up of the problems of extrasensory perception and the PK effect. Much of the "evidence" for the survival hypothesis could be explained more conservatively by extrasensory perception and by the psychokinetic ef-

fect than by supposing the survival of personal agencies. The question was *how much?* To answer this question required more knowledge of extrasensory perception and PK than was available—perhaps still more than we have as yet.

But now that we have tasted gains, however small, there is real encouragement to get on with the job and especially to attack the larger problems that have been looming in the background as we have gone about these preliminary studies. We know there *must* be much more yet to discover, if only to explain the findings already established. It will require a grand extension of understanding indeed to include and account for the things we have already found out. To complete the task remaining will take a long series of inquiries, extending possibly through generations to come.

And these newer areas to be filled in on the map of human personality will be interesting whether or not they coincide with current religious views of man! They will be more valid and reassuring than all past charts have been, and their net guidance value to human conduct and happiness will reach new levels of effectiveness such as the older and more familiar sciences have already brought in their application to the problems of human life.

J. B. R.

A CONSISTENT STANDARD OF SIGNIFICANCE

THE criterion of statistical significance most commonly used in articles in this JOURNAL has been a critical ratio of 2.5, which represents a probability of .0062. In recent years the frequent application of methods other than critical ratio tests of significance has required the use of the standard of significance common to those methods; namely, a probability of .01. In explaining the term "significance" in the Glossary, we have cited both standards.

Dr. T. N. E. Greville, who has recently joined the editorial staff, suggests that the use of the two standards (sometimes in the same article) is confusing and unnecessary since both can be expressed as probabilities. He proposes that the JOURNAL adopt one or the other probability level for significance as a fixed policy.

In carrying out Dr. Greville's suggestion, we simply affirm editorially what has already taken place in practice. Throughout the

research of the Parapsychology Laboratory, probability values have been, at least implicitly, the basis for experimental conclusions. Although CR = 2.5 has been a convenient criterion, in recent work frequent use of methods requiring a P = .01 criterion has naturally led to borderline CR's being stated in probability values and conclusions drawn with respect to the P = .01 criterion.

In choosing between the probability associated with a CR of 2.5 and P = .01 as a standard, we do not have to be arbitrary. The P = .01 criterion has become almost universally accepted in present-day statistical work, and many useful tables are published with reference to this standard. On the other hand, the CR = 2.5 criterion is an approximation to a "four times the probable error" standard of fifteen years ago that is today almost forgotten.

The actual effect of this fixed standard will be slight. Previously CR's that have been between 2.33 and 2.50 have occasionally been characterized as of "borderline" significance since they met the P = .01 criterion but not the CR = 2.5 criterion. With the P = .01 standard any CR of 2.33 or greater (when the deviation being tested is in the direction expected by hypothesis) can be designated as significant.

The foregoing is a clarification of editorial policy. Statistical standards are a research tool, and must be left, of course, to the final judgment and responsibility of the individual investigator.

C. E. S.

* * * * *

The editorial staff of the JOURNAL has been considerably strengthened recently by the following changes:

Dr. Charles E. Stuart, who has returned from Stanford University to the Parapsychology Laboratory, Duke University, has resumed the larger share in editorial responsibility which he held in 1942.

Dr. T. N. E. Greville, Actuarial Mathematician in the United States Bureau of the Census, has joined Lt. Joseph A. Greenwood, U.S.N.R., as co-editor of the mathematics and statistics involved in the JOURNAL.

Miss Betty M. Humphrey, of the Parapsychology Laboratory, Duke University, has been added as Assistant Editor.

The names of all three persons mentioned have appeared in the pages of the JOURNAL and are doubtless familiar to our older readers.

Our editorial policy, always a delicate thing to define and defend, is undergoing serious discussion at present. A number of issues are concerned, one of these being the advisability of printing our technical reports in somewhat less technical language. Whatever modifications of policy may be adopted, the views of our readers regarding them will always be of great interest to us.

ESP AND INTELLIGENCE

By Betty M. Humphrey

ABSTRACT: This is the second installment of Miss Humphrey's report of the Earlham College experiments in ESP. The case for ESP is exceptionally strong for these two series, since there was very significant evidence, not only in the score totals, but also by several other independent methods of evaluation. The present report deals with the question of whether the ability to score high in ESP tests is correlated with high ratings in intelligence tests. The analyses show that there is a positive correlation between these test scores, but it is not enough to establish a definite conclusion.

Three other series of ESP test records obtained at Duke University were used to supplement the Earlham College research. These also gave significant evidence of ESP; but only one showed a significant correlation between ESP scoring and intelligence. The conclusion based on the entire study is that a positive relation is suggested but not established.—Ed.

Progress in science is made by the discovery of the relations of the phenomenon under study to other better-known processes. The finding of a consistent relationship between ESP ability and any mental state or trait, temporary or permanent, would be important. Not only would it increase understanding of the nature of ESP, but it might also give an increased degree of control over the phenomenon.

The relationship of ESP ability to certain more or less transient conditions, such as those caused by drugs and toxins, has already been studied by some experimenters. For example, small doses of alcohol were found to increase the ESP scores in one series; in another the subjects scored much lower after a large dose of sodium amytal than they had before the drug was given. Investigators have frequently noted in the course of their experiments that illness seems to have a detrimental effect on their subjects' scoring ability.

On the other hand, the relationship of ESP ability to the more permanent characteristics or conditions of subjects has received relatively little attention so far. Frequently, when an experimenter has described a specific subject, he has given an estimate of his intelligence or has pointed out certain abilities or other distinguishing characteristics. Yet little has been done in an experimental way to study the personality traits associated with good ESP subjects.

Of the few experimental studies dealing with ESP and personality, several have been made on psychotic subjects. For example, Shulman (7) found that a group of manic-depressive depressed subjects gave significantly positive results while involutional melancholia patients gave low scores (an average of 4.72 in 290 runs, where 5.00 is expectation). Van Wiemokly (7) working completely independently, obtained a very similar average score for involutional melancholia patients (4.77). Price (6) found that a group of manic-depressive depressed patients gave a higher average score than other types of psychotic patients. While the tests with the involutional melancholia subjects gave a positive deviation from expectation in her experiments, their averages were nevertheless among the lowest obtained. Price also found that when the patients were rated according to attitude as being either cooperative, apathetic, or irritable, they ranked in this same order with regard to success in the ESP tests.

Although there are no reports of experiments with normal subjects in which an attempt was made to get at the problem of a possible association of ESP ability with certain mental characteristics, there do appear in the literature from time to time comments which are pertinent to this question. For example, it was noted by Rhine that one of his major subjects was an exceptionally bright student, a fact which probably indicates that he had above average intelligence. Drake's high-scoring subject (2), diagnosed as a hypothyroid case, had very low intelligence as was shown by an I.Q. of 55 on the Stanford Revision (1915). Most of the Duke experiments with college students as subjects have given significant results. It is known that in general such a group has higher than average intelligence.

The report which comes the closest to giving actual experimental evidence on the problem is that of Bond (1), in which a rank difference correlation was computed for ESP averages and intelligence ratings for a group of retarded children. However, this correlation was done as a side issue to her main experiment, and in it she made use of intelligence ratings which are of doubtful value. Since she made the analysis some time after her experiment was concluded, achievement test averages were not available for some of her subjects. Therefore, teachers' judgments were utilized for some subjects

and achievement test scores for others. Thus it seems unwise to base any conclusions on this evidence. Since the correlation reported was $+.14 \pm .14$, the figure carries small suggestive value.

From these facts and impressions, it is not possible, of course, to draw any conclusions about the relations of ESP and intelligence. Nor do we gain much insight into the personality requirement for the successful subject. Such a state of indecisive knowledge led me to initiate a series of experiments in 1938 which were continued until 1941, when the press of other matters interrupted the experimentation. Since continuation of the experiments is uncertain, it seems well to report the results obtained so far in the hope that they may be of some suggestive value to others who are free to continue the research.

The possible relationship between ESP ability and intelligence will be discussed on the basis of evidence from several studies in which the subjects' ESP scores were compared with their ratings on intelligence tests. In a later paper a comparison of the ESP scores and personality test ratings of these same subjects will be reported.

If a study of the relationship between ESP and intelligence is to be meaningful, it is necessary to have adequate assurance that ESP has been functioning in the series involved. Thus only significant ESP series have been considered in the following report. The two series done at Earlham College (Richmond, Indiana) during the years 1938-40 form the main basis of this study. It has been possible to obtain supplementary material, however, in similar studies of three other series: one by Pratt and myself in 1941, and two which were originally conducted for other purposes by other experimenters. From the two institutions at which the latter series were done, I was able to obtain the official records of the intelligence ratings for the subjects involved.

The Earlham College Series

General

The ESP results of the first experiment, which was done at Earlham College, have already been reported in detail (3). In the first of the two series making up this experiment the GESP technique was used. In these tests the subject tried to identify the card

at which one of the experimenters was looking. In Series II, however, the tests were done according to the screened BT procedure in which the cards were at all times handled out of sight of both the subject and the experimenters. When the total of 3,480 runs of both series are pooled, a highly significant CR is obtained—one with a probability of approximately .0002.

Further assurance that such results can be explained only by ESP and not by any alternative hypothesis is found in several other significant figures obtained from the analysis of the data for position effects. Statistics applied to the hit distributions in the run (RSR) and in the segments of the run (SSR) revealed that significant relationships obtained ($P = .002$). (See Glossary for terms.) Also, declines were noted which were in keeping with those commonly found in ESP and PK scoring. For example, there was a significant difference between the scoring rate in the first half of the run and that of the last half ($P = .0047$). Moreover, since the first report on this experiment was published, further analyses have revealed additional position effects with significant differences which will be reported in greater detail in a separate note. The Earlham work, then, constitutes an exceptionally well-established case for ESP.

Correlation Analysis

For 13 of the 14 major subjects in Series I it was possible to obtain from the college files the percentile ranks on the A.C.E. (American Council on Education) Psychological Examination for College Freshmen, 1937 edition. This test is designed to appraise "scholastic aptitude," or general intelligence, with special reference to the college curriculum. Although the test is not to be construed as a precise measurement of intelligence, it is said to be generally indicative of the student's "mental alertness."

By Pearson's product-moment method, the percentile ranks of the 13 subjects were correlated with their average number of hits per run in the ESP tests. The resulting correlation coefficient is $+.27$, which is insignificant for the small number of cases involved.

Because of the inconclusive nature of this result, more extensive research was necessary. When Series II was completed, I therefore gave all the subjects the A.C.E. Psychological Examination for

College Freshmen, 1939 edition. I was thus able to get separate ratings for the two parts of the test and the total gross score as well. (Only the gross scores had been available in the college files for Series I.) One part of this test deals with "linguistic ability," while the other appraises "quantitative ability," that is, ability to think in mathematical terms. The scores on these two sections combine to give a gross score which is said to be indicative of "general intelligence" or "scholastic aptitude." As in the first series, the average run scores of the subjects were correlated with the gross A.C.E. test ratings. The correlation coefficient is +.34. Since 33 subjects were involved in this correlation, the probability[1] for such a coefficient is .05.

When the ESP averages are correlated with the "linguistic ability" test scores,[2] the correlation coefficient is +.43 This result is significant, having a probability of approximately .01 ($N = 33$).

The correlation of ESP averages and the "quantitative ability" test scores gives a negligible coefficient of +.09.

Contingency Tables and Chi-Squares

As an additional measure, a 2×2 contingency table was worked out for the two series in this experiment. In this analysis, subjects above and below the group median in gross intelligence ratings were divided into subjects scoring above and below the chance mean in the ESP tests. The frequencies in each category were then tabulated. The resulting fourfold tables were as follows:

	Ser. I			Ser. II			Tot.		
	+ESP	−ESP	Tot.	+ESP	−ESP	Tot.	+ESP	−ESP	Tot.
High Intell. Percentile	6 (a)	0 (b)	6	13 (a)	3 (b)	16	19 (a)	3 (b)	22
Low Intell. Percentile	3 (c)	3 (d)	6	7 (c)	9 (d)	16	10 (c)	12 (d)	22
Total	9	3	12	20	12	32	29	15	44

[1] The probabilities for this and all other correlation coefficients in this paper were obtained from R. A. Fisher's *Statistical Methods for Research Workers*, Table V. A., p. 188.

[2] The manual for the test states that the "linguistic ability" tests have generally higher correlations with scholastic aptitude than do the quantitative. Other studies comparing this test with other mental tests have noted the same tendency. Super, for example, reported in 1940 (8) that the "quantitative ability" scores on this

If perfect positive correlation existed between high intelligence and ESP scoring, the cases would all fall in the diagonal cells *a* and *d*. In the present instance, a chi-square test[3] gives a P of .012. This marginally significant figure suggests that there is some degree of association between the two test measures.

Correlation Studies of Other Experiments

The results of the Earlham experiment suggested that some relationship—even though it is too small for predictive purposes—may exist between ESP ability and "mental alertness" as measured by the test given. On the basis of these findings further inquiry into the problem seemed advisable. During the two years following the Earlham series, I was able to gather data on three more experiments. One was a series done by Pratt and myself in the course of a comparison of five ESP techniques. The other two series were ones which had been done for other purposes by members of the Parapsychology Laboratory.

Chutes Series

Of the five series reported by Pratt and myself in 1941 (**4**), the only significant evidence of the functioning of ESP was given by a matching procedure in which enclosed cards were dropped through chutes. The significance of this series was due to a large *negative* deviation. Various hypotheses for this significant rejection of the target symbols were discussed in the original report of the ESP results. The 21 subjects were given the Henmon-Nelson Test of Mental Ability, Form B, which is designed to test the mental ability of college students. The correlation of the percentile ranks of these subjects with their average ESP scores gave an insignificant coefficient of $-.09$.

An analysis of this series into a 2×2 contingency table gives the following distribution of cases:

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test were not suitable for the purpose for which they had been devised, "whereas high 'linguistic' scores are indicative of general academic ability." Thus the correlation of ESP test scores with the "linguistic" test ratings is perhaps the more indicative figure since the gross test ratings have been diluted by the inclusion of the unsuitable "quantitative ability" test scores.

[3] Yates's correction for small numbers of cases has entered into this computation.

	+ESP	−ESP	Tot.
High Intelligence Percentile	2 (a)	8 (b)	10
Low Intelligence Percentile	6 (c)	4 (d)	10
Total	8	12	20

Thus it can be seen that the majority of cases fall in the diagonal cells *b* and *c*. This suggests that in this extremely negative series the more intelligent subjects scored negatively. The number of cases involved in this series is small, however, and the chi-square obtained from the fourfold table is insignificant ($\chi^2 = 1.86$).

Price-Pratt Series

The Price-Pratt Series, as reported in this JOURNAL in June, 1938, dealt with the effect of the subject-experimenter relationship on ESP scoring. For 26 of the subjects, it was possible to obtain I.Q.'s based on the Stanford-Binet Scale. (Scores for the other subjects were not available.) These subjects gave a significant positive deviation on the two screened matching tests used in this series. The resulting CR is 3.22, which adequately excludes the chance hypothesis as an explanation of the results.

The I.Q.'s were correlated with the subjects' average run scores, as in previous series. The coefficient of correlation in this case is +.48, which is significant for the number of cases involved.

Division of the cases into a 2×2 table gives the following result:

	+ESP	−ESP	Tot.
High Intelligence	10 (a)	2 (b)	12
Low Intelligence	5 (c)	7 (d)	12
Total	15	9	24

After applying Yates's correction for small numbers of cases, we get a chi-square of 2.56, which is insignificant.

Pratt-Woodruff Experiment, Series B

For the sake of completeness of reporting, it is necessary to mention one other small set of observations. It was possible to obtain percentile ranks for performance on the A.C.E. Psychological Examination for 16 of the 32 subjects in Series B of the Pratt-Woodruff experiment (**5**). These records were taken from the files of Duke University; only the gross performance percentile for each subject was available. The percentiles for subjects who took the test in different editions were equated by means of charts published by the A.C.E. Inspection of the intelligence ratings for this group showed a very skewed distribution. Six of the 16 subjects were in the top ten per cent. No subject was below the 50th percentile.

The correlation of these percentile ranks and average ESP scores gave a coefficient of −.15. This is an insignificant figure since it is just a little over half of its standard deviation.

A 2×2 contingency table presents the data in the following form:

	+ESP	−ESP	Tot.
High Intelligence Percentile	5 (a)	3 (b)	8
Low Intelligence Percentile	5 (c)	3 (d)	8
Total	10	6	16

The chi-square for this distribution is exactly zero.

Summary of Correlation Studies

The correlation results for the five series are summarized in the following table:

Table 1
RESULTS OF ALL CORRELATIONS OF INTELLIGENCE AND ESP SCORES

SERIES	TEST	N	*r*	P
Earlham, Series I	A.C.E. (1937)	13	+.27	Insig.
Earlham, Series II	A.C.E. (1939)			
	linguistic ability	33	+.43	.01
	quantitative ability	33	+.09	Insig.
	gross score	33	+.34	Insig. (.05)
Chutes	Henmon-Nelson	21	−.09	Insig.
Price-Pratt	Stanford-Binet	26	+.48	.01
Pratt-Woodruff, Series B	A.C.E. (various editions)	16	−.15	Insig.

Chi-squares on all fourfold tables were insignificant with the exception of that for the total Earlham experiment.

DISCUSSION

The probability column in Table 1 shows that two of the coefficients are significant. The "linguistic ability" test scores of the A.C.E. Examination correlate significantly with the ESP averages of the 33 subjects in Series II. Also, the ESP score averages of the 26 children in the Price-Pratt Series are positively correlated with their intelligence ratings. These coefficients suggest that subjects with higher intelligence ratings give more evidence of ESP ability; and conversely, that subjects with high ESP ability tend to have higher intelligence ratings.

The other coefficients are insignificant, although one is suggestive. Most of the coefficients are positive; that is, the more intelligent subjects tended to make the better ESP scores. Of the two exceptions to this generalization, one is the case of the negative Chutes Series. It must be remembered that in this series the *negatively* scoring subjects are the ones who contributed the evidence of ESP ability. This fact, together with the reversal of the more "typical" ESP-intelligence relation, suggests that actually the Chutes Series may be in line with the rest of the series. Certainly a larger number of cases is needed before any generalization is warranted.

The second series showing a negative correlation is that by Pratt and Woodruff. This series is notable for the small range of intelligence ratings involved. As was noted previously, there was no

subject under the 50th percentile rank. Actually, 13 of the 16 subjects had percentile ranks between the 70th and the 100th percentiles. With such a distribution as this we would not expect to get much differentiation—there were no subjects with low intelligence.

The two significant correlation coefficients found in this study suggest that some small but positive relation between ESP ability and general intelligence exists. The amount of relationship between these two test scores is certainly not large enough for predictive purposes. Nor is the evidence at hand conclusive enough to justify at this stage any speculations as to the possible *meaning* of such a correlation.

The methods used in this study have been adequate for exploratory purposes—they have served to indicate that further research in this area is feasible. But only by the use of more precise measures of intelligence and by more extensive studies can we confirm or correct the suggestion given by this study that there is a relation between ESP test scores and intelligence test ratings.

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AN EXPLORATORY INVESTIGATION OF THE PK EFFECT

By CHARLES J. HERTER and J. B. RHINE

ABSTRACT: The Herter experiment on the PK effect described in this article may be submitted as one of the considerable number of researches that seem to meet all of the requirements of sound evidence. The tests were carried out with only one subject, who threw six dice from a cup onto a padded dice table for a total of 594 runs of four throws each. There were equal numbers of runs for all six faces. In all, 207 hits above expectation were obtained. The odds against the occurrence of this deviation by chance are over 100,000 to 1. The question of whether recording errors or skilled throwing and the like might have produced these results is met by the analyses for the effect of position on scoring rate. When the records of the 36-throw sequences used in this series were examined, a remarkably consistent decline from beginning to end of the sequence was discovered. When these 36-throw sequences were divided into halves, a significant difference in scoring rate was found between the first and second halves. This and other differences rule out the various counterhypotheses as explanations of the results and constitute the strongest kind of evidence for the PK hypothesis.

The experimenter, Mr. Charles Herter, was an undergraduate student of Psychology at Duke University when the work was performed. He is now in the United States Navy.—Ed.

THE EXPERIMENT which is reported in this paper was the outcome of an effort by one of the authors, C.J.H., to satisfy himself by personal investigation as to the reality of the occurrence of the PK effect. The experiment was carried out in the summer of 1942 in the Parapsychology Laboratory at Duke University under the general guidance of Dr. C. E. Stuart who was in charge of the Laboratory at the time. Mr. Charles Stoddard (hereinafter referred to as C.S.), with whom C.J.H. was conducting some ESP tests at the time, was subject in the experiment. C.S. had shown ability as an ESP subject, and it was thought worth while to try him in PK tests as well, especially since he showed considerable interest in them.

The experiment which is reported here represents all the PK work done by C.J.H. and C.S. during the summer period. The records were turned over to J.B.R. on his return to the Laboratory in the fall, and the analyses and evaluations reported below were

made out by him. A complete recheck was made later on by Miss Betty M. Humphrey.

The Experiment

The experimental work was carried out during the period from July 7 to August 5, 1942. C.J.H. was present throughout as observer and recorder. C.S. was the only subject taking part in the experiment, and for the most part no other persons were present during the tests, although occasionally there were visitors for short periods.

In all there were 11 rounds, each round consisting of the throwing of six dice 36 times for each of the six faces of the die. This makes a total of 2,376 throws of six dice each, or 14,256 single die throws. Stated in terms of the standard run of 24 die throws, these represent 594 runs.

When six dice are thrown at a time, the expectation is that on the average one hit per throw will occur for a specified target face (that is, the face indicated in advance as the objective of the test). Earlier work has indicated that six dice per throw is a favorite number and one that yields relatively high scores. It has the advantage of giving the subject a measure in a short time of how well he is doing, and this immediate check-up goes far toward enlivening the test. At the same time, six dice are not too many for easy observation and recording.

The first ten rounds, or 540 standard runs, were intended to constitute a research unit complete in itself, and the eleventh round was the beginning of a second experiment. However, shortly after the eleventh round was completed, the subject left school. Accordingly, it seems best to pool the eleventh round with the first ten, since the one extra round would not be sufficient in itself to warrant separate treatment.

A set of six small-sized white dice were used throughout the series. They measured 7/16 of an inch on the edge and were marked with black spots produced by paint inserted in small excavations. They were ordinary commercial dice of plastic material. The equal number of throws for each face served to control any effect of bias due to faulty structure of the dice.

The throwing of the dice was done with a cylindrical container of about one quart capacity. The dice were first shaken and then

thrown onto a padded table. The observer, C.J.H., read the dice after each throw and called aloud the number of hits obtained. The records were made in a routine form and consisted of the sequences of throw scores or hits per throw. There were 36 entries for each successive target face of the die from one to six, 216 entries for each round.

At first the subject was asked to begin with the one-face as target and follow the normal sequence up to the six-face. Later he was allowed to begin with whichever face he preferred and to choose the order of faces to be followed. However, the records were kept in the same order as had been originally adopted.

The record sheets did not accommodate all 36 entries made for each target face in a single column; in fact, C.J.H. did not use a regularly spaced form with ruled columns. Instead, plain paper with three broad-ruled vertical spaces was used, and 36 entries were made in each space, as a rule with 25 entries in one column and 11 more in a second column.

When the records were turned over to J.B.R. for analysis, they were subjected not only to the routine rechecking for arithmetical accuracy and the usual statistical evaluations but also to an analysis for distribution of hits according to position of the throw in the test. At the time of the experiment, C.J.H. knew nothing of the position effects which were being found at about that time in the analysis of earlier PK data. In fact, at the time C.J.H. began his experiment, such effects had not yet been discovered to be a general feature of PK test results. Thus, when he turned over his records to J.B.R. in the autumn of 1942, he had no reason to anticipate that they would reveal any position effects. This independence is of some value in itself.

The remainder of the report contains two essential parts: first, the results of the experiment as conducted by C.J.H.; second, the analysis for position effects and internal differences independently carried out on the data by others.

Results

Total Scores

The total of 11 rounds, or 594 standard runs, gave 2,583 hits or coincidences between the dice thrown and the target face. The

expectation on the theory of chance was four hits per run, or 2,376 for the whole series. This leaves a positive deviation of 207 and an average score of 4.35. The standard deviation for the whole series is ±44.47, and the CR is 4.65, which is highly significant. The odds against its chance occurrence are around 100,000 to 1. The sample is complete and is ample in size. The results are not explainable by chance.

It may be of interest to some readers to observe the distribution of scoring over the range of the six target faces for the 11 rounds. These score data are given in the Appendix table at the end of the report. The total deviations for the different faces are as follows:

Face	One	Two	Three	Four	Five	Six	Total
Deviation . . .	+27	+22	+37	+68	+20	+33	+207

The four-face gave almost twice as great a deviation as the next highest target face. The six-face, which is most often preferred and which is most suspected of being favored by structural inequalities, ranks third.

Position Effects

The entire series was subjected to an examination of the hit distribution in the 36-throw sequence made for each target face. For this purpose each sequence of 36 throws was divided into halves, thirds, and sixths, and the distribution of hits in each subdivision was found for the entire research. Thus we have cross-sections of the distribution of successes for the entire results, with the 36-throw sequence subdivided into segments of 18, 12, and 6 throws each. The results of these analyses are stated in Table 1 in terms of total deviation (that is, hits above or below the mean expectancy from chance) for each segment.

The results of the six-throw segments are given in Part A of the table in terms of total deviations from expectation (expectation would be 66 hits) for the six-entry segments. The table gives the results for the segments separately for each face of the die and for all faces combined. It will be noted that there is a general trend of decline for all but the five-face.

In the totals for the six-throw segments in the 36-throw sequence, there is a moderately graded decline through the first five segments: +94, +36, +34, +17, −6. Then, the final segment, with

a +32, gives a distinct terminal salience effect. The *two* end segments (first and last) give a total deviation of 126, while the *four* interior segments total only 81. Such salience effects have been mentioned in earlier reports; for example, in the first Reeves study on PK (2), in the large Gibson Series (1), and the Frank Smith Series (3).

Table 1

VERTICAL DISTRIBUTION OF HITS FOR THE INDIVIDUAL TARGET FACES

A. SIX-THROW SEGMENTS

Order of Segment	One-Face	Two-Face	Three-Face	Four-Face	Five-Face	Six-Face	Total
1.....	+16	+15	+15	+22	+ 5	+21	+94
2.....	− 3	− 3	+ 9	+14	+ 5	+14	+36
3.....	+ 2	+ 6	+14	+ 2	− 1	+11	+34
4.....	+ 5	+ 2	+ 7	+ 9	− 4	− 2	+17
5.....	+ 3	0	− 7	+ 7	+ 2	−11	− 6
6.....	+ 4	+ 2	− 1	+14	+13	0	+32

B. TWELVE-THROW SEGMENTS

Order of Segment	One-Face	Two-Face	Three-Face	Four-Face	Five-Face	Six-Face	Total
1.....	+13	+12	+24	+36	+10	+35	+130
2.....	+ 7	+ 8	+21	+11	− 5	+ 9	+ 51
3.....	+ 7	+ 2	− 8	+21	+15	−11	+ 26

C. EIGHTEEN-THROW SEGMENTS

Order of Segment	One-Face	Two-Face	Three-Face	Four-Face	Five-Face	Six-Face	Total
1.....	+15	+18	+38	+38	+ 9	+46	+164
2.....	+12	+ 4	− 1	+30	+11	−13	+ 43

It is in Parts B and C of the table, however, that the declines stand out most clearly. In Part B all targets except the five-face show marked declines. So also do the deviations in the "Total" column in Part B, which are as follows: +130, +51, +26. The difference between the first and second segments here gives a CR of 2.18 and that between the first and third gives a CR of 2.86. Likewise, in Part C, the difference between the deviations of +164 and

+43 for the first and second segments as shown in the total is quite significant; its CR is 2.72.

It is evident from these declines, distributed as they are over five of the six target faces, that some other factor than chance exercised its influence on the distribution of hits and that success was quite consistently less likely in the second half and in the third 12-throw segment than it was in the first. Not only is the PK effect strongest in the initial segment of the 36-throw sequence, but in the very first throw it shows *twice* as large a deviation as in any of the other 35. The first six throws of the sequence give positive deviations from expectation as follows: +40, +20, +19, +18, +6, +1.

There is no noteworthy trend in the temporal course of the experiment. The chronological decline found in many earlier experiments does not appear here. The total deviations for each round are as follows:

	July	July	July	July	July	July	July	Aug.	Aug.	Aug.	Aug.	Total
Date . .	7	9	10	13	14	16	17	3	3	4	5	
Round .	1	2	3	4	5	6	7	8	9	10	11	
Dev. . .	+19	+24	+42	−10	+17	+19	+5	+41	+29	+29	−8	+207

The total deviation of the first five rounds is +92 while that of the second five is +123. It may be of passing interest, however, to note that there are two periods to the experiment, the first from July 7 to July 17, and the second from August 3 to August 5, with an interval of over two weeks between. Subdividing the two actual work periods into halves (omitting the middle odd score of −10 according to practice), we get the results shown in the following table:

	First Half	Second Half
First Experimental Period	+ 85	+41
Second Experimental Period	+ 70	+21
Total	+155	+62

This can be taken only as a suggestive item in the general study being made of position effects, but if time or chronology plays a role in success curves in PK tests, such a gap as occurred in the present series is of importance.

Discussion

In view of the plan of the experiment under which equal numbers of throws were made for the different faces of the die, the significant results obtained cannot in any way or degree be accounted for by the hypothesis of faulty dice. Any faults the dice may have had would have been equalized in their effects because of the technique of throwing "around the die." The use of skills in throwing the dice also was guarded against by the throwing of six dice at a time and by the fact that the dice were always thrown from a container onto a special dice table.

The only one of all the hypotheses that might be relevant to these results is that of error. Fortunately, however, it is possible to get at even this hypothesis in a conclusive way by means of the analysis for position effects. The analysis for relative success according to position in the throw sequence of the test should show no particular distributional differences because of recording or other errors. Such errors would not be localized. Rather, any such position effects found must be attributable to the PK effect itself.

In the significant differences introduced above in connection with the decline effects, we have, then, not only a valuable and quite independent basis for the rejection of the chance hypothesis, but also a control that rules out the hypothesis that the results could have been due to errors. As a matter of fact, the discovery of significant position effects in these data effectively covers a number of counter-hypotheses regarding the adequacy of the experimental controls—points such as the question of faulty dice, skilled throwing, inaccuracy of observation, recording, and the like. Indeed, there is no known weakness of the experimental procedure which would conceivably discriminate in favor of score success in the first half as against the second. No physical characteristic of the dice, no skill in throwing, and no weakness in the clerical work would produce a decline as the test advanced through the sequence of 36 throws for each target face, only to incline again in the last segment of six throws.

Rather, we have to turn for an explanation of these primacy and finality effects, this terminal salience, to psychological hypotheses. Such distributions are, of course, familiar both in the ESP

and the PK work. We therefore attach meaning to the position effects beyond the mere verification value they have for the PK process itself. The relatively uniform way the distributions appear in this study manifests an impressive degree of lawfulness in the process at work.

There is a further semispeculative remark that may be of interest to fellow-experimenters in parapsychology. Past experience indicates that the most favorable conditions for a successful experiment were present here. It was the first and only experience with PK testing for both C.J.H. and his subject. Both were deeply interested, curious about PK, and eager to see if it really worked and would work for them. Such an attitude is believed to be highly favorable, and under those conditions PK did work. But this does not necessarily mean that C.J.H. and C.S. could duplicate the result, for their motivation would not now be the same; and this observation, all psychologists will agree, is a datum of the greatest importance at the present stage of the inquiry.

Appendix

SCORE TOTALS BY ROUNDS FOR ALL TARGET FACES

ROUND	Date	TARGET FACE						TOTAL DEV.
		One	Two	Three	Four	Five	Six	
1.....	7/7	42*	40	41	30	42	40	+19
2.....	7/9	36	42	50	36	38	38	+24
3.....	7/10	48	40	46	52	32	40	+42
4.....	7/13	27	28	30	40	38	43	−10
5.....	7/14	34	35	33	50	40	41	+17
6.....	7/16	44	39	31	37	37	47	+19
7.....	7/17	29	37	34	54	34	33	+ 5
8.....	8/3	44	44	38	46	41	44	+41
9.....	8/3	40	40	51	34	39	41	+29
10.....	8/4	38	36	52	46	42	31	+29
11.....	8/5	41	37	27	39	33	31	− 8
Total Score		423	418	433	464	416	429	
Total Deviation.....		+27	+22	+37	+68	+20	+33	+207

Grand Total:		
	Runs	594
	Hits	2,583
	Dev.	+207
	SD	± 44.47
	CR	4.65

*Expectation for each face, representing 36 throws of six dice, is 36; hence this score of 42 has a deviation of +6.

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FURTHER POSITION EFFECTS IN THE EARLHAM COLLEGE SERIES

By Betty M. Humphrey

ABSTRACT: Miss Humphrey here continues the account of the Earlham experiment on ESP of which two reports have been given. This "chapter" tells of the significant patterns found in further analyses of the data—patterns due to the effect which position of the trial has upon success in scoring. The effect of time is evident in the marked chronological decline found in each series. Here for the first time the quarter distribution (QD) of the page, which has played a prominent part in the analysis of the PK results, is introduced in the study of ESP data. Although these ESP series showed a distribution different from that typical of the PK researches, there were quite significant differences between certain quarters of the page.—Ed.

In a previous report in this Journal (1) I described the ESP results of the two series conducted during the years 1938 to 1940 at Earlham College (Richmond, Indiana). The study was designed primarily to approach the question of a possible relation between ESP ability and intelligence; in another paper in this issue, this phase of the research is reported. Since the first report on the ESP results obtained in the Earlham investigation, further analyses of the data have been carried out which have revealed additional evidence of position effects, an account of which follows.

Series I consisted of tests made under the GESP technique with two experimenters present at each session. This series gave a highly significant CR of 3.48. In Series II the subjects were tested by means of a screened BT technique. The total deviation in these tests was positive, but the CR was not quite significant (CR = 2.20). When the deviations for these two series are pooled, the resulting CR is 4.00.

In addition to these significant figures as evidence of ESP, the first paper reported a significant covariation of salience ratios (P = .002) and a significant CR of the difference between the first half and the second half of the run for both series pooled. The analysis upon which the latter figure is based showed that the entire significance of this experiment rested upon the deviation amassed in the first half of each run.

Similar decline effects have been receiving attention in the current series of reports of the PK researches. Other similarities between the findings in the ESP and PK researches led to the analysis of the two Earlham series by one of the methods common in PK research. The method consists of a "quarter distribution" (QD) study to determine the scoring rate in the four quarters of the record page. Throughout the PK reports and in a recent survey of the QD's of all available PK series, the predominant pattern of hit distribution was that in which the first or upper left quarter of the record page showed the greatest number of hits, and the fourth or lower right quarter the lowest.

When the record pages of the two Earlham ESP series were analyzed by quarters, the deviations were found to be distributed as follows:

Series I	Series II	Total
336 runs per quarter	429.6 runs per quarter	765.6 runs per quarter
+73 | +124	+18 | +147	+89 | +271
+33 | + 30	-23 | + 35	+10 | + 65
$CR_{d(2-3)} = 1.76$	$CR_{d(2-3)} = 2.90$	$CR_{d(2-3)} = 3.34$

Contrary to what one might have predicted on the basis of the PK researches, the third quarter has the largest deviation in both ESP series. In Series I both of the quarters on the lower half of the page have only small deviations, while in Series II the second quarter is lowest, being the only negative quarter on the page. In the pooled total for the experiment, the difference between the second and third quarters is highly significant (CR = 3.34). Since on the basis of ESP researches alone we have no grounds for expecting any particular distribution of hits,[1] this CR of the difference must be corrected for selection from a number of other possible

[1] Another question one can ask about the distribution of hits is whether, given the total hits observed, there is significant variation among the quarters. Supposing that the scoring rate is equal in all four quarters, a chi-square fit of the observations to this supposition can be made. The resulting chi-square in the case of the total Earlham experiment has a probability of .006, which demonstrates that the variation in scoring in these four quarters is significantly different from chance.

combinations of two quarters. The probability associated with a CR of 3.34, however, is so small (P = .0004) that, after appropriate correction, it would still be highly significant (P = .005).

The only other comparable ESP evidence at hand is the previously unpublished quarter distribution for the precognition series (PDT) reported by Humphrey and Pratt in 1941 (2). This series had given a positive but insignificant total deviation. At the time the Earlham series were being analyzed for the QD, a similar study was made of these precognition record pages. The following QD was found:

91.2 runs per quarter

+8	+43	+51
-37	+13	-24

$CR_{d(2-3)} = 2.96$

It can be seen at a glance that this distribution is closely similar to those of the two Earlham series. The third quarter has the largest number of hits, while the second quarter has fewest—in this case it is even quite negative. The CR of the difference between the two quarters is 2.96.

Many more analyses will be needed to determine whether or not such a hit distribution is common to ESP series generally. As an aid to future study, it might be helpful to record certain similarities and differences in the series which have shown this hit patterning. The three series were conducted under widely different procedures: (1) a GESP technique wherein telepathy, clairvoyance, or both might function; (2) a BT clairvoyance technique; (3) a precognitive (DT) technique. This variety suggests that the hit pattern of the QD's is not dependent upon the use of any particular procedure.

All three series had two features in common—the *same type of cards and record sheets.* The latter were the usual printed ESP record pads with double lines after each group of five calls. In the two Earlham series the subjects were acquainted with the form

of the record sheets, but they did not record their own calls nor look at the sheet during the run. On the other hand, the subjects in the PDT series recorded their own calls for the 10 runs on the page without interruption. (Only after the full page was finished were the actual card orders determined.) These differences would seem to indicate that the manner of recording is not responsible for the QD's. There still remains the possibility that the form of the record sheet may be an important factor in determining the hit distributions.

Although it seems unimportant at this stage in our knowledge, another common factor in the three series was the experimenter. In the two Earlham series co-experimenters worked with me in each session, while in the precognition series, I was usually alone with the subject. (However, after the subject had been instructed in the task, he worked alone at filling in the runs on the page.)

A very important difference between these ESP series and the PK researches should be pointed out. In ESP series such as these there is no opportunity for the subject to know whether he is succeeding or failing until the run (or the page in the PDT series) has been finished. On the other hand, the subject in the PK tests can instantly check on his success with each throw of the dice. Throughout the test period he is able to keep a fairly accurate idea of his performance with reference to chance. It may be that his constant adjustments to the knowledge of success and failure play a very important role in the production of the hit distributions so familiar throughout the PK researches. The absence of such knowledge may be the factor responsible for these ESP hit patterns. It will not be a difficult matter to devise experiments to test out the influence of these factors.

By way of summarizing the results of the QD analyses, perhaps the most important points to observe are, first, that all three series showed a *vertical decline*—that is, a decline within the run; and, second, that all three researches also showed *horizontal inclines*—that is, a rise in scoring from left to right on the record page. These two trends combine to produce a QD which has the highest scoring rate in the third quarter and the lowest in the second. We need more analyses to discover whether this QD pattern of the present

report is typical of ESP in general or is merely an odd occurrence peculiar to the three series involved.

* * * * *

The two Earlham series were also analyzed for the chronological distribution of hits. In Series I each subject completed 10 pages, or 100 runs. Usually one page was finished in an experimental session. If we pool the first five pages (50 runs) for each subject, we find that the total deviation for the first half of the series was +234 for the 700 runs. The pooled total for the last five pages

Table 1
CHRONOLOGICAL DISTRIBUTION OF HITS FOR BOTH SERIES

	First Chronological Period		Second Chronological Period		CR_d	χ^2
	Runs	Dev.	Runs	Dev.		
Series I.....	700	+234	700	+28	2.75	7.56
Series II.....	720	+196	710	+ 4	2.47	6.10
Total.....	1,420	+430	1,410	+32	....	$\Sigma\chi^2$=13.66 d.f.= 2 P= .0011

of each subject gave 700 runs with a deviation of only +28. (See Table 1.) The CR of the difference between the two chronological periods is 2.75, which is significant.

In Series II each subject finished five pages, or 50 runs. Thus, in order to compute the first-half vs. the second-half differences for the series, it is necessary to omit the middle or third page of each subject's data. The first 20 runs of each subject gave 720 runs with a deviation of +196, while the last 20 runs of each gave 710 runs with a deviation of +4. (See Table 1.) The CR of the difference for these two temporal periods is 2.47.

* * * * *

The significant CR's of the difference found in the reanalysis of the Earlham series constitute exceptionally strong evidence for ESP. The finding of internal differences which were not anticipated by the subjects or the experimenters at the time of the experiment has the value of a completely independent confirmation. Evidence

of this nature should be particularly reassuring to those who still hesitate to accept the ESP hypothesis.

For those who are convinced of the occurrence of ESP and who are now primarily interested in the nature of this phenomenon, every new evidence of lawfulness, such as these position effects, offers a possible—even a promising—doorway to a greater understanding of it. It will be of special interest to continue to observe the similarities and differences in these position patterns as they appear in the ESP and PK research data, and in general to relate these to the broader realm of position effects in the more familiar cognitive processes such as memory.

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MINOR ARTICLES AND NOTES

[Under this heading will occasionally appear briefer publications having value and interest for our readers but being in some respects less complete or less conclusive than our major articles. —Ed.]

THE EFFECT OF ALCOHOL UPON PERFORMANCE IN PK TESTS

By RICHARD L. AVERILL and J. B. RHINE

ABSTRACT: An exploratory experiment was carried out to discover whether alcohol affects performance in PK tests. Two subjects participated, one of them, however, being unable to retain the alcohol long enough for much of it to be absorbed. The other subject showed a great drop in scoring after ingesting the alcohol, so great that a significant difference was obtained.

This research is the first one to be reported in which the tests were based on the throwing of 96 dice at a time.

Mr. Averill was a Graduate Assistant in Psychology at Duke University when the experiment was conducted. He is now Director of Classification at the Washington State Penitentiary, Walla Walla, Washington.—Ed.

THE EXPERIMENT herein reported deals first with the question of whether PK occurs; that is, whether the mental states of certain subjects under certain conditions are able to bring about an effect upon the movement of physical objects (dice) without known intermediating connections. In addition, there are two new problems which are dealt with: that of the effect of the narcotic drug, ethyl alcohol, upon the performance of the subjects in PK tests, and the question of whether the PK effect can be demonstrated upon as many as 96 dice thrown at a time. In earlier reports of PK experiments, mention was made of throwing 1, 2, 3, 6, 12, and 24 dice at a time, but 24 was the maximum.

Thus far there has been little study of the conditions affecting the performance of the subject in the PK tests. For the most part the tests have been directed toward ascertaining whether physical variations in the experimental situation—such as size of dice or number per throw—would affect the results; and only secondarily have conditions been discussed which might affect the efficiency of

the subject. The report by Price and Rhine (5) indicated that the social factor of the relation between subject and experimenter is important. And Stuart and Gibson (3) found that a strikingly negative deviation resulted from working the subject in darkness. Important as these findings are, it is evident that there is need of much more experimental manipulation of the subject himself with a view to discovering more about the nature of the PK process as well as acquiring greater control over it. It is with this objective in mind that the present research dealing with the effect of alcohol upon performance in PK tests was carried out.

There is no need to review here the general evidence of the physiological and psychological action of alcohol. It is perhaps sufficient to recall that in large enough quantities alcohol acts as a general narcotic, and that in addition to reducing sensory acuity it adversely affects perceptual, rational, and motor judgments. In the studies of ESP, the relatively small dosage of alcohol used by Brugmans (1) appeared to increase the scoring success of the subject, but in the comparable experiments conducted in the Parapsychology Laboratory and later confirmed at New York University by Clark and Sharp (2), sodium amytal, which has a similar physiological and psychological effect, markedly interfered with success in ESP tests when taken in moderately large doses. It is well known that the effect of narcotics when taken in very mild doses is often, temporarily at least, contrary to the effect of large doses. In mentioning the effect of amytal and alcohol upon ESP, we make the assumption that there is probably a close relation between ESP and the PK effect, as has been previously emphasized (7). Granting this assumption, we should expect a large dosage with a narcotic drug to have the same effect on the PK process as on ESP.

The research reported here, as far as the drug aspect goes, is an exploratory one. While we believe the experimental precautions are quite adequate, the administration of the drugs represents only a preliminary approach to this problem. A number of conditions required for an ideally controlled drug experiment were lacking; for example, the calculation of correct dosage in proportion to body weight and the disguising of the drug to be administered.

Other experiments followed in which these conditions were more adequately provided; but the investigation we are reporting here

served a useful purpose in justifying the greater labor of these later, more systematic studies.

This experiment, then, was a "feeler," and it came in the midst of a more or less spirited production of exceptional PK results which accompanied a state of group enthusiasm lasting over a period of weeks. During that time a number of variations were introduced of which the alcohol experiment is but one. Reports of others will follow.

Description of the Experiment

On the morning of the twelfth of May, 1936, the authors, together with Mr. A. J. Linzmayer, the secretary of the Laboratory, were engaged in an informal discussion of the PK research and the factors which might influence success in scoring. The informal and off-the-record throwing of dice which punctuated the discussion gave the experimenters an indication that all three were in a favorable mood for doing PK work. After weighing a number of suggestions as to what might best be done on such an occasion, we agreed that a test of the effect of alcohol was needed and would appeal to all concerned as the most interesting of the various possibilities. The only conveniently available alcohol supply, it was found, was in the form of gin. It was thereupon decided that gin cocktails would be the form of treatment given the subjects. Since it was agreed that large doses would be administered, all thought of concealing the nature of the drink from the subject was dismissed. It was frankly recognized that a psychological factor might be added to the primary physiological effect of the alcohol, but all agreed that as subjects we would try to do our best before and after the drug was administered.

Since one of the three participants needed to remain sober in the interest of reliable recording, it was decided that all three would run through the pre-alcohol control tests and the two who obtained the highest averages would be given the gin cocktail. These preliminary tests consisted of 20 throws of 96 dice each, or the equivalent of 80 runs of 24 single die throws for each subject. As will be seen below when the data are presented, A.J.L. was eliminated and the authors were the subjects.

In earlier tests involving comparisons of different numbers of

dice per throw we had found that the relative rates of scoring were quite as high with 96 dice per throw as they were with smaller numbers; and since the rate of accumulating data was much faster, this procedure had come into favor with both subjects and experimenters. It was therefore selected for use in this experiment.

The dice were white with black spots. They measured 7/16 of an inch on the edge and were made of plastic material. No check on the trueness of the dice is regarded as necessary, since the *comparison of two conditions in which the same dice were used* furnishes the control needed to rule out the hypothesis of faulty dice.

As in most earlier researches, the six-face was the face for which the dice were thrown throughout this experiment. The attitude of the subject was supposedly one of attempting to will or volitionally to influence the dice to come up sixes. It was realized by all that the alcohol would probably interfere with the PK effect; yet, as far as could be determined, each subject made a genuine effort to achieve high scores after as well as before taking the alcohol.

The dice were thrown from a box with a sliding bottom, which was set on an inclined board above the end of a special dice table. The catch was operated by a string which, when pulled, dumped the dice onto the inclined board down which they rolled to the table below. As the dice fell on the padded table, they bounced and rolled over an area of about 18 square feet and lay on the black background until they were counted and the record was made. The sixes were counted by the observer. They were picked up carefully and set apart for a recount at the end of the session. The subject himself was not allowed to touch the dice until the counting was completed and the record made. Throughout the experiment, both before and after the alcohol was administered, there were in effect two observers present besides the subject. After the alcohol, of course, one of these was not in the best condition for making accurate observations, though he was still able to distinguish the dice and to take an interest in the scoring.

Each of the two subjects was given 100 cubic centimeters of gin with a like amount of ginger ale. Following the administration of the alcohol, a period of twenty minutes was allowed before the start of the experimental series. Then at least 20 throws, or 80

runs, for each subject were to be conducted, more if the subjects were able and willing to continue. As it turned out, 25 throws, or 100 runs, were made by each of the two subjects. One of the subjects, R.L.A., was unable to retain his dose throughout the experimental series, and vomited just after the fourteenth of the 25 throws. After four more throws, he vomited again, and expressed the view that he was relatively sober, that he felt the effect of the alcohol was not great. The other subject, J.B.R., retained the alcohol until after the experimental series was completed, but it had not been completely absorbed, for he too was compelled to part with a portion of it soon thereafter.

Results

Altogether there are 440 runs in this series. They represent 110 throws of 96 dice each. The total scores give an excess of 104 hits above chance. This positive deviation of 104 has a significant CR of 2.72. The results are definitely not due to chance.

The total of 440 runs includes the 80 runs made by the subject, A.J.L., who failed to qualify for the gin cocktail. He obtained only five hits above expectation on the 80 runs of the control series. Since R.L.A. received 25 above and J.B.R. 63 above, A.J.L. became the observer from that point on. Excluding his 80 runs and taking only the pre-alcohol and post-alcohol tests of the other two subjects, we have 360 runs with a deviation of +99 and a CR of 2.86.

Table 1

Total Results of PK Tests Before and After Alcohol

Subject	Pre-Alcohol				Post-Alcohol				CR_d
	Runs	Dev.	Av. Score	CR	Runs	Dev.	Av. Score	CR	
R.L.A.	80	+25	4.31	1.53	100	+24	4.24	1.32	.26
J.B.R.	80	+63	4.79	3.86	100	−13	3.87	.71	3.36
A.J.L.	80	+ 5	4.06	.31					
Total...	240	+93	4.39	3.29	200	+11	4.06	.43	1.89

Total Both Conditions:		
	Runs	440
	Dev.	+104
	Av. Score	4.24
	SD	± 38.28
	CR	2.72

The individual and total results of the tests before and after the alcohol are given in Table 1 together with the CR's, average scores, and CR's of the differences. When the scores for both subjects are pooled, there is a significant CR of 2.54 for the difference between the pre-alcohol and post-alcohol scores. The CR of the difference when A.J.L.'s results (only pre-alcohol) are included is 1.89. The CR of the difference of J.B.R.'s work alone may be seen to be 3.36. Further details may also be observed in the Appendix. It gives the actual scores of the two subjects before and after the alcohol was administered.[1]

Discussion

Evidence of PK

The significant CR's reported in Table 1 exclude the chance hypothesis as an explanation. We may also dismiss the hypothesis that the dice could have been mishandled or thrown so as to favor the six-face. The wholly mechanical manner of releasing them obviates this possibility. Likewise we may conclude that the results are not explainable in terms of faulty dice since the analysis of the data offers a sufficient control on the dice. The CR of the difference between the pre-alcohol and the post-alcohol scoring for subject J.B.R. is 3.36. This CR has a probability of .0004, which may be corrected for the selection of but one of the three subjects without losing its significance. (P would be equal to .0012.) No imperfections in the dice could account for this effect.

There seems to the authors to be no reasonable alternative to the acceptance of this result as indicating that the presence of some factor other than chance, faulty dice, or the manner of throwing the dice is demonstrated.[2] No other hypothesis could logically be applied to the data other than that which the experiments were de-

[1] A subsidiary question—whether the differences may be attributed to increased variability in the post-alcohol work—can be answered best by noting the observed standard deviations. For the pre-alcohol trials the SD was 3.90; for the post-alcohol trials the SD was 3.60, with a chance expectation of 3.65. In the case of the work of J.B.R. the SD's were 4.60 and 3.61 respectively. The CR of the latter difference based upon these observed SD's was 2.85.

[2] Perhaps the hypothesis of counting errors should be regarded as reasonable. But the idle subject witnessed the counting, and in the case of R.L.A., who was but little affected by the alcohol, this secondary observation offers considerable reassurance since the main point of the paper rests on J.B.R.'s work.

signed to test—namely, that the mind of the subject influenced the fall of the dice.

Effect of Alcohol

Striking though the effect of the ingestion of alcohol appears to have been on J.B.R.'s scoring rate, it is well to interpret the results with some caution. The lowering of the score average (4.79 to 3.87) *could* have been due to suggestion or unconscious desire to produce such an effect. But in either case the effect is of considerable interest at this early stage of the PK research, especially in showing the responsiveness of the PK process to experimental factors. It will not be difficult to distinguish by means of further experiments between the effect of the drug and the result of suggestion.

Number of Dice Per Throw

It has been shown in this experiment that a very large number of dice can be subjected to the PK effect in a single throw. The average score of 4.24 for the entire series compares favorably with the average scores made with smaller numbers of dice per throw in other series. If, as would be entirely proper, we take as a basis of comparison only the 240 runs which the three subjects made in the pre-alcohol stage, the average of 4.39 for the test with 96 dice per throw makes a still more favorable comparison. This average may be compared with the 4.26 given by the 12 series of experiments summarized by Rhine and Humphrey in their study of the quarter distribution of the page (**6**). But in view of the fact that the various experiments entering into this comparison were based upon the use of different dice, it will be better to compare the results of the tests with 96 dice per throw with those made with the same size and type of dice thrown for the same target face, but with only *two* dice thrown at a time as reported in the Small and Medium Dice Series (**4**). The average score for the small dice in this series was 4.54, which is .15 above the average for the pre-alcohol tests with 96 dice per throw. This is a disproportionately small difference in score average for so great a difference in the number of dice thrown at a time. Even so, all that can be said here is that PK seems to have about the same general order of effectiveness in tests with 96 dice per throw as in tests with small numbers. As a first

case of the 96-dice test, it is enough to warrant some interest. More conclusive comparisons are needed.

In any event there is no conclusion to be drawn from the use of 96 dice per throw which has not already been drawn from the use of 6 or 24 per throw. Unless it affects the subject's attitude, number per throw has thus far appeared to be of no importance in determining scoring rate.

The following general remark may be of passing interest. We have noted that the subjects, once they are led up to the throwing of 96 dice at a time, seem to like the feeling of "larger dividends" that results. After the greater number is used, going back to a small one seems extremely tedious and slow. The work with 96 dice was done at a time of mounting enthusiasm when increasingly large numbers of dice at a throw were being tried. When a large number was introduced with due challenge, the appeal to the subject to see what could be done was strong. It is our impression that this challenge to the spirit of achievement in the subjects was a most provocative factor in bringing about the PK results. However, no mere verbal description of this aspect of the experiment can be counted on to tell another experimenter just how to go about reproducing the motivation. Some will readily catch the knack of creating this working mood and others perhaps will fail with the best of instructions.

Position Effects

The records of the hits were made in short sequences of five entries each. This sequence of five throws made up a unit of performance, changes of subject being commonly made after each such sequence. The scoring, too, was computed on that basis.

When the data were rechecked by Miss Betty M. Humphrey in 1943, it had become a standard practice to look for position effects in the results. The five-throw sequences were the only "structure" that provided material for analysis for position effects. They showed a small vertical decline, taking the data as a whole, from a deviation of +55 for the first and second throws to one of +34 for the fourth and fifth. The distributions of total deviation per throw are given below in Table 2.

Table 2
VERTICAL DISTRIBUTION OF SCORING IN THE FIVE-THROW SEQUENCES

ORDER OF THROW	PRE-ALCOHOL*		POST-ALCOHOL		TOTAL*	
	Runs	Deviations	Runs	Deviations	Runs	Deviations
1.....	48	+ 7	40	+15	88	+22
2.....	48	+29	40	+ 4	88	+33
3.....	48	+20	40	− 5	88	+15
4.....	48	+11	40	+ 1	88	+12
5.....	48	+26	40	− 4	88	+22

*Including A.J.L. pre-alcohol.

The most interesting position effect is the difference between the distribution for the pre-alcohol scores and the post-alcohol. The alcohol did not cause a drop in the first throw but did in the other four. This suggests that in the initial throw of the sequence the inhibiting effect of the alcohol could generally be thrown off, but *only* for a single trial in each sequence.

CONCLUDING REMARKS

The experiment shows that the PK effect can occur when 9¢ dice are thrown at a time and the results suggest that alcohol inhibits the PK effect.

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Appendix

Scores per Throw of 96 Dice Made in PK Tests Before and After Alcohol
(Sixteen hits are expected on the average as a result of chance alone.)

	Before		After	
	R.L.A.	J.B.R.	R.L.A.	J.B.R.
	15	20	19	16
	26	22	16	18
	17	19	16	15
	21	12	20	11
	16	27	20	14
	19	12	18	19
	22	16	14	20
	14	18	15	15
	10	20	22	14
	16	9	16	9
	15	19	15	19
	14	19	15	19
	18	18	14	20
	20	18	14	16
	12	29	19	20
	16	22	21	13
	15	21	20	13
	19	24	13	12
	17	19	23	19
	23	19	12	17
			21	14
			22	7
			15	20
			11	11
			13	16
Throws	20	20	25	25
Runs	80	80	100	100
Deviation	+25	+63	+24	−13

160 runs: +88
CR = 3.81

200 runs: +11
CR = .43

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THE PK EFFECT: THE FIRST DOUBLES EXPERIMENT

By Margaret Pegram Reeves and J. B. Rhine

ABSTRACT: This article is about a somewhat different type of PK test than has been reported. It describes another one of the very early PK experiments, the second by Margaret Pegram Reeves. It is unique in that it is based on the throwing of a pair of dice for "doubles"—double sixes in 369 runs (of 12 throws each) and double ones in 364 runs. The scoring was better for double sixes than for double ones, and was significantly above chance in the two sections taken together. A satisfactory control excluded the possibility that the results could be due to faulty dice, and significant internal differences render skilled throwing of the dice an unlikely explanation. These results are much less striking than, for example, those of the other Reeves series. But they are marginally significant and hold special interest because of the extremely low chance of a hit (one in 36 throws) which applies to the doubles test.

Mrs. Reeves was a senior student in Psychology at Guilford College (N. C.) when she conducted the experiment. In addition to her work on the PK effect she has published several research reports on ESP.—Ed.

We offer herein a brief account of an investigation carried out by M.P.R. while she was a senior student specializing in Psychology at Guilford College, North Carolina. Her objective was to test the PK hypothesis by means of dice-throwing procedures then in use at Duke University. The special feature of her experiment was the choice of double sixes and double ones as her targets in two series involving the throwing of a pair of dice.

All the tests on which this report is based were conducted between February 10 and May 14, 1934. They were made in M.P.R.'s home with M.P.R. as the sole subject throughout and with no one else present except an occasional visitor. The dice used were ¾-inch cubes of the common commercial variety made of white plastic material with black spots. They were shaken in cupped hands and thrown on a smooth blanketed surface with a backstop from which they were made to rebound.

M.P.R. recorded the total of the upturned faces of the two dice for each throw (single face records were not kept). The records were made in a bound composition book in the following form: The upper half of the page was devoted to the Double Sixes Series and the lower half to the Double Ones. The recording was done in

columns of 12 entries each, and three columns were bracketed together as a set. There were six or seven sets (that is, 18 or 21 columns) to the page for each series. The checking and scoring were done at the end of each set. The reason that the set was taken as a unit is that the expectation from chance is one double (for the face specified in advance) for each set of 36 throws of the pair.

The first target for a given session was usually double sixes. All the sixes on a page would be completed before beginning on ones. It was customary, too, to complete a page or several pages at a given session and to make an equal number of throws for double sixes and double ones. The interruptions to this schedule were minor and unavoidable.

The results were rechecked and analyzed in 1943 under the supervision of Miss Betty M. Humphrey of the Parapsychology Laboratory. The hits for the target doubles were counted, and a control check was made for the number of doubles of the *opposite* face thrown in each series. These results furnish a control on the question of imperfections of the dice.

The distributions of hits in the three columns of the set and in the sets on the page were also determined in order to discover whether the data showed a horizontal decline of success from left to right across the page similar to that found in other PK researches. The data were examined, too, for distribution of hits within the column itself with a view to discovering whether declines occurred. The analysis for position effects, incidentally, brought out differences of sufficient magnitude to eliminate concern over questions of dice bias, skilled throwing, recording errors, and the like.

Results

The Chance Hypothesis

There was a total of 369 runs or columns of 12 throws in the Double Sixes Series, and 364 in the Double Ones, making 733 in all. (See Table 1.) This represents 244.3 sets (of 36 throws). From these 8,796 throws of the pair of dice, 283 doubles were obtained for the face specified, which is 38.67 above expectation from chance alone. This gives a CR of 2.51 and the corresponding probability of .006 that such a result would be produced by chance alone. These figures are regarded as significant of extrachance

causation and will be taken as a sufficient basis for the discussion of other hypotheses. It will be noted as we proceed that other significant internal relations are evident and that the evidence against the chance hypothesis accumulates somewhat with the presentation of further analyses.

Table 1

TOTAL EXPERIMENTAL RESULTS OF BOTH SERIES

SERIES	RUNS	HITS	TOT. DEV.	SD	CR
Double Sixes.....	369	151	+28.00	±10.93	2.56
Double Ones.....	364	132	+10.67	±10.86	.98
Total.....	733	283	+38.67	±15.41	2.51

A comparison of the figures for the two series shows that the double sixes give a significant CR of 2.56 while the double ones are insignificant. The total deviation of the double sixes is almost three times that of the double ones.

The Hypothesis of Faulty Dice

The results which are next in order of importance, once the issue of chance is settled, are those which concern the question of the trueness of the dice. As a control, the Sixes Series was checked for double ones, and the Ones Series for double sixes. These control checks should furnish evidence as to whether the dice were themselves physically distorted so as to favor the face for which they were thrown. Since both series show positive deviations (see Table 1), then we should expect the control checks to give similarly positive deviations if the results are due to distorted dice. On the contrary, as the figures in Table 2 show, negative deviations were obtained on both.

Table 2

CONTROL CHECK ON DICE

SERIES	RUNS	HITS	TOT. DEV.	SD	CR	CR_d
Double Ones in Sixes Series...	369	120	− 3.00	±10.93	(−) .27	
Double Sixes in Ones Series...	364	115	− 6.33	±10.86	(−) .58	
Total Control.....	733	235	− 9.33	±15.41	(−) .61	2.20
Total Experiment.....	733	283	+38.67	±15.41	(+)2.51	

The difference between the number of doubles counted in the experimental trials and the number counted in the control gives a CR of 2.20, or 2.16 by the arc sine method.[1] This is a highly suggestive rather than a significant figure, but on the strength of it there is reasonable ground for confidence that faulty dice did not account for the successes summarized in Table 1.

But more conclusive evidence is given below in Table 3 to show that imperfections in the dice cannot furnish an adequate explanation for the deviations obtained. There we find differences between the runs in the set that are not to be explained by chance. When the sets of each series are superimposed, the analysis of the pooled results shows that in both series the subject did poorly on the first run of the set and obtained all her positive scoring on the second and third. In fact, there is a significant difference between the first and second runs when the two series are pooled (CR = 2.88; or, by the arc sine method, 2.76), and between the first and third runs the difference is almost significant (CR = 2.25; or, by the arc sine method, 2.20). After appropriate corrections,[2] the CR of 2.88 has a probability of .008. This significant result could not be due to faulty dice, and accordingly we reject that hypothesis.

Table 3

Distribution of Hits in the Runs of the Set

Run	Double Sixes			Double Ones			Both Series Pooled			CR_d
	Sets	Hits	Av. Hits per Set	Sets	Hits	Av. Hits per Set	Sets	Hits	Av. Hits per Set	
1st....	41	41	1.00	40.67	32	.79	81.67	73	0.89	$CR_{d(1-2)}$ = 2.88
2nd...	41	60	1.46	40.33	49	1.21	81.33	109	1.34	
3rd...	41	50	1.22	40.33	51	1.26	81.33	101	1.24	$CR_{d(1-3)}$ = 2.25
Total.	123	151	1.23	121.33	132	1.09	244.33	283	1.16	

[1] Greenwood, J. A. The evaluation of differences of success ratios from binomial populations. *J. Parapsychol.*, 1943, **7**, 277-80.

[2] These differences are not in the direction we would have anticipated them to be on the basis of other researches. Rather, a decline from the first to the second and third runs would be a more typical horizontal distribution. But we recognize that there is a great difference in the expectancy per trial between this and other reported experiments (1/36 for this series and 5/12 or 1/6 for the others). This is the only research with so small an expectancy per trial, and it is regarded as a case apart until other instances arise. It is proper to require a somewhat higher standard of significance for an *incline* effect when a decline effect would have been expected on the basis of the work most nearly approximating this. The CR of

The Hypothesis of Skilled Throwing

From the presentation of results up to this point, then, it may be safely concluded that there are extrachance factors involved and that these are not inherent in the physical structure of the dice themselves. The next question is: How have the dice been influenced? Have they been influenced in the process of being thrown—that is, through manipulative skills on the part of the subject who handled them? For the answering of this question we must again go into the analysis of the results with respect to the position of the run in the set as presented in Table 3.

The significant difference between the first and second runs of the set for the work as a whole can hardly be explained by skilled throwing, for such skill would presumably improve over the session and the record page if not over the experiment as a whole rather than rise and fall with the structure of the set. This recurrent patterning is impressive when we observe (in Table 4 below) the consistency of the incline from the first to the second run in the set extending over the seven sets on the record page. Delicate skills may be variable, especially if unconscious in character. But no such patterning as this is known in the exercise of skills, whereas position effects in PK tests have been observed in numerous experiments in which skills could not possibly have operated—tests in which the dice were thrown by cup or by machine. The occurrence of a special response to set structure is now well established in PK. The decline in scoring rate shown in Tables 5, 6, 7, and 8 argue further against the skill hypothesis. The long training involved should not be expected to produce a decline in scoring.

Position Effects in General

In addition to the analyses yielding the position effects within the set as given in Table 3, others were made on the data of these

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the difference of 2.88, with its corresponding probability of .002, needs to be multiplied by two, giving .004, to take care of this correction. An additional correction may be made to cover the fact that there was a selection of the difference between the first and second runs. This would give a P of .008, which is still significant. We think we need not consider the possibility of a difference between the second and third runs since this difference has not entered into the analyses of position effects and is not likely to be of interest in that connection. But even if this unlikely possibility is assumed, the corrected P still closely approximates the 1% standard of probability ($P = .012$).

Table 4

DISTRIBUTION OF HITS FOR THE THREE COLUMNS WITHIN EACH OF THE SETS

	Column	Set 1		Set 2		Set 3		Set 4		Set 5		Set 6		Set 7	
		Runs	Dev.	Runs	Dev.	Runs	Dev.	Runs	Dev.	Runs	Dev.	Runs	Dev.	Runs	Dev.
Double Sixes	1	19	−2.3	19	− .3	19	− .3	19	+1.7	19	− .3	18	+3	10	−1.3
	2	19	+1.7	19	+1.7	19	+6.7	19	+1.7	19	+2.7	18	+4	10	+ .7
	3	19	+ .7	19	+7.7	19	− .3	19	− .3	19	+ .7	18	−1	10	+1.7
Double Ones	1	20	+1.3	20	−2.7	20	−2.7	20	+ .3	19.3	−3.4	16	−1.3	6	0
	2	20	+6.3	20	+2.3	20	−2.7	20	+2.3	19.3	−4.4	16	+1.7	6	−1
	3	20	+3.3	20	−1.7	20	+3.3	20	+1.3	19.3	+4.6	16	− .3	6	0
Total	1	39	−1	39	−3	39	−3	39	+2	38.3	−3.8	34	+1.7	16	−1.3
	2	39	+8	39	+4	39	+4	39	+4	38.3	+2.2	34	+5.7	16	− .3
	3	39	+4	39	+6	39	+3	39	+1	38.3	+5.2	34	−1.3	16	+1.7

two series. For some reason, perhaps the unusual probability of success of 1/36 per throw, there is much less distributional differentiation than there was, for example, in M.P.R.'s other work with the high-dice and low-dice tests.[3]

We present, first, in Table 5 the comparative scores for the sets from left to right on the record page for each series separately and for the two series pooled. In view of the very small probability of a single hit in a set, these differences are more meaningful than the figures at first suggest. The first set, with an average of 1.28 for the two series pooled, is considerably above the expectancy of 1.00. A greater contrast is found when the first three sets on the left of the page are compared with the last three on the right. For the double sixes the averages respectively are 1.26 and 1.21; for the double ones, 1.12 and 0.99; both togther, 1.19 and 1.11. Although they are not great, these differences are in the expected direction and are of interest in the general interpretation of declines in the PK research data.

There is little to be said about the scoring distribution in the column except that there is only an unimportant decline from top half to bottom half in one series and no difference in the other. (See Table 6.)

[3] Reeves, Margaret Pegram, and Rhine, J. B. The PK effect: II. A study in declines. *J. Parapsychol.*, 1943, **7**, 76-93.

Table 5

SCORE AVERAGE OF THE SETS ON THE PAGE

ORDER OF SETS LEFT TO RIGHT	1	2	3	4	5	6	7
Double Sixes.....	1.00	1.47	1.32	1.16	1.16	1.33	1.10
		1.26				1.21	
Double Ones.....	1.55	0.90	0.90	1.20	1.03	1.00	0.83
		1.12				.99	
Total.....	1.28	1.18	1.10	1.18	1.10	1.18	1.00
		1.19				1.11	

Table 6

HIT DISTRIBUTION IN COLUMNS IN TERMS OF THE TOTAL DEVIATIONS OF THE HALF-COLUMN

	DOUBLE SIXES		DOUBLE ONES		BOTH POOLED	
	Total Hits	Total Deviations	Total Hits	Total Deviations	Total Hits	Total Deviations
Top Half.....	76	+14.5	66	5.33	142	+19.83
Bottom Half....	75	+13.5	66	5.33	141	+18.83

The quarter distribution (QD) for the page has been obtained for each of the two series and for the two series pooled. These, too, fail to show significant differences, and the QD's are unlike those generally found to be typical of singles and high-dice experiments. There are as yet no other doubles distributions for comparison.

Table 7

QUARTER DISTRIBUTION ON THE PAGE FOR SIXES AND ONES

(The figures show total deviation per quarter page.)

DOUBLE SIXES
358 runs

+11.17	+1.17
+ 7.17	+7.17

DOUBLE ONES
354 runs

+6.5	- .5
-1.5	+7.5

BOTH SERIES POOLED
712 runs

+17.67	+ .67
+ 5.67	+14.67

To complete a report of the hit distribution analysis there remains the chronological distribution. We present in Table 8 the score averages of the two series separately and pooled for the chronological subdivisions of the experiment. The score averages are given for the data divided into four chronological subdivisions, and also divided into two.

Table 8

CHRONOLOGICAL DISTRIBUTION IN EACH SERIES AS A WHOLE WHEN DIVIDED INTO TWO AND FOUR SUBDIVISIONS

(The figures represent average number of hits per set.)

PERIOD	DOUBLE SIXES			DOUBLE ONES			BOTH SERIES		
	Sets	Four Subdiv.	Two Subdiv.	Sets	Four Subdiv.	Two Subdiv.	Sets	Four Subdiv.	Two Subdiv.
1.....	28	1.46	1.30	28	0.93	1.08	56	1.20	1.19
2.....	28	1.14		31	1.23		59	1.19	
3.....	31	1.19	1.16	30	1.07	1.09	61	1.13	1.13
4.....	36	1.14		32.3	1.11		68.3	1.13	

The small decline observed in the pooled averages is due to the Sixes Series and is not significant.

DISCUSSION

The conclusion that these results are extrachance in character is based upon CR's that have emerged from the analyses. None of them are strikingly large but they suffice to make the experiment worthy of interest, and the special character of the tests—the use of doubles with 1/36 probability of success per throw—affords a contribution in itself to the wealth of the PK evidence. This report cannot, and need not, be regarded as one of the main pillars of the structure of evidence, but the significant CR of the difference shown in Table 3 cannot, in our judgment, be accounted for except by the PK process.

Likewise, the various analyses for position effects have yielded different and less striking results than have most of those previously reported. But they are the first to be presented as coming from doubles tests, and it may turn out to be their uniqueness in this regard that contributes their principal importance.

In general there are two main queries that call for discussion. The first is: Why does the three-run set in this experiment not show

a typical decline as all the rest have done? What is there in the situation about the doubles test that might explain this departure from the usual? Until further studies are made, the answer can be only hypothetical, but it occurs to us that the result may be due to the small probability of getting a hit. There is apparently some attitude of mind with which the subject begins the set which has to be dissipated before PK can operate. It may be something of the nature of excessive effort or a too violent resolution arising from the feeling (right or wrong) that doubles are hard to get with an expectancy of but one in 36 throws. A test of this hypothetical notion will itself represent an interesting experiment. (It might be arranged by throwing for "triples" with three dice and keeping to a set structure similar to that used by M.P.R. for purposes of comparison. A still greater incline effect might be expected with this harder test.)

The other question that is especially raised by these results is: Why are there more hits in the Sixes Series than in the Ones? It is very easy, of course, to say this higher scoring on the higher faces is simply the influence of imperfections in the dice. Even if it be adequately determined, as we believe it is in both experiments, that the statistical significance of the results is not due to such physical factors, we cannot say that the dice used were "perfect." But there is no need to establish such a point since the significant CR of the difference of 2.88 in Table 3 is sufficient evidence of the extrachance character of the results; and, as we have explained, this difference could not be a result of imperfections in the dice. So there may be inequalities in the dice which, along with the PK effect itself, have to be reckoned with. Perhaps the same amount of PK working on double ones produces less than one-half the positive deviation that it does on double sixes because of bias in favor of sixes and, of course, against ones. We cannot settle this issue either, although there are relevant facts that will help us to reach a tentative decision. From Table 8 it will be seen that it was only in the first 84 runs, or 28 sets, that there was any appreciable superiority on the part of the sixes. The experiment began in the first chronological period with an enormous advantage in favor of the sixes, but it ended with the sixes and ones close together. In the second period the ones even went ahead, with 1.23 as their average score per set and 1.14

for the sixes. Then followed a reversal in favor of the sixes, and the last period finished with the averages for the two faces almost equal (1.14 and 1.11).

Moreover, turning back to Table 3 and examining the progress through the set, we see the same trend. In the first run of the set, double ones went badly below chance, dropping to an average score of 0.79. They remained well below the sixes through the second run, but on the third emerged to a score of 1.26 while the sixes were only 1.22. Now this suggests, although it does not prove, the operation of *psychological* factors rather than physical ones.[4] So also does the fact that the rise of .46 (from 1.00 to 1.46) from the first to the second run in the set for the sixes is very close to the rise of .42 for the ones in the same comparison. This position effect is more plausibly a psychological phenomenon (and is more significant statistically) than anything else in the report.

It will be interesting to follow this question of preferences versus dice inequalities through the sequence of further publications that bear upon it and to discover whether the somewhat consistent discrimination against the lower faces of the dice is clearly and unmistakably demonstrated to be psychological in origin or whether it is to some extent due to physical factors in the dice. As we have explained, it is not essential to an evaluation of the data as evidence of the PK effect, but it nevertheless represents an interesting problem in its own right.

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[4] In a letter which M.P.R. wrote to J.B.R. during the course of the experiment, she expressed a difference in her attitude toward the two target faces, indicating a preference for sixes. In the Double Ones Series she found it harder to concentrate on ones and felt "inclined to try to avoid double sixes rather than to try for low numbers."

BOOK REVIEW

A PSYCHIATRIST APPLIES A THEORY OF ESP

By C. E. Stuart

Dr. Laurence J. Bendit, a psychiatrist, observed among the statements of his patients a number of instances of what seemed to be extrasensory perception. He has reported some of these in a small volume entitled, "Paranormal Cognition, its place in human psychology."[1] As indicated by the subtitle, the essay deals mainly with an attempt to explain how and why paranormal cognition occurs and the relation between that occurrence and aspects of personality of the patient. Dr. Bendit sets for himself the task of explaining this for the benefit of the practicing psychiatrist whose business it is to understand completely the psychology of his patient, yet whose training does not include discussions of parapsychology.

The practical situation is represented in Dr. Bendit's Case 2.

> Case 2. Miss A., a middle-aged woman, was an undoubted hysteric who complained of being troubled by spirits who caused much physical discomfort and pain by their antics. Moreover, she was bothered day and night by voices. On enquiring what they said, it appeared that, as a rule, they spoke of people by name and commented on events. These people and events seemed to have no relationship to the patient and she could not make out why these matters should be thrust at her. Further enquiry tended to show that what she "heard" was almost always about such things (e.g., the death of a village magnate) as might well be talked about in her neighbourhood, or such as she might have read about in the newspapers. The obvious conclusion was to attribute the physical symptoms to characteristic hysteria and to call the spirit voices illusions. On one occasion in the consulting-room, however, a clue was suggested. The patient affirmed that, even as she was speaking to me, the voices were dinning in her ears. I enquired what they were saying, and she repeated several names. "They're talking about somebody called Wright, who is ill. Now the names Robert, Marjorie . . ." and so on. I remembered that at lunch, an hour or so ago, we had spoken of several friends who had these names, and moreover that we had spoken of Wright's health. The patient, of course, was miles away at the time.[2]

[1] London: Faber and Faber, 1944. Pp. 79. The essay was originally presented as a medical thesis at Cambridge University.

[2] *Op. cit.*, p. 31.

Was what the voices were saying the result of extrasensory perception or a dramatization of an unrecognized internal conflict? Dr. Bendit proposes that paranormal cognition was partially responsible for the experience. He reports successful alleviation of distressing symptoms when patients could be adjusted to the reality of their over-sensitivity to extrasensory perception.

Dr. Bendit's proposals regarding the psychology of paranormal cognition start with a distinction between "paranormal cognition" as the psychological process and what Professor Thouless terms "psi" as the receptor mechanism (a distinction comparable to that between vision as a psychological process, and the eye as a receptor). Paranormal cognition, Bendit says, is not necessarily extrasensory. When it is considered as a general extension of the sensitivity of an individual there is a range in which the phenomena cannot be separated from hyperacute sensory reactions or unusual intuitive insight.

Dr. Bendit recognizes two main axes or gradients of this sensitivity:

> 1. That between the levels of physical perception of gross and material objects at one end, through a twilight zone, where both are represented, to levels of pure psychical cognition of non-physical conditions (e.g., emotional states).
>
> 2. That between blind, automatic or reflex response to extrasensory mandates, to that of controlled and epicritic[3] cognition of non-physical conditions, with willed and deliberate activity in response to it.

Since perception of unknown or ambiguous objects and experience of uncontrolled impulses are the typical initiators of morbid anxiety, Dr. Bendit adopts the procedure of teaching the patient to understand the unusual but rational source of extrasensory perception in order to deprive the experiences of their threatening character.

He says that the person "who is sensitive, intuitive, and artistic on the one hand, or else unreasonably suggestible and unstable on the other" is most likely to evidence paranormal cognition; and since "it is the perceptive, highly strung person who tends to come into the psychotherapists' hands, the probabilities are that a very fair fraction of psychological patients have the Psi function as a more or less active and significant though unintegrated part of their make-up."

[3] Finely localized or discriminating.—Ed.

It is proposed that the psi function occurs early in the evolutionary process and has the undifferentiated, grossly impulsive quality of instinct. Western culture, Bendit says, has tended toward the development of individuality, with the resultant over-emphasis of finely specialized discriminations and repression of instinctive urges. The psi function becomes pathological when it falls behind the highly developed capacities for discrimination of the rest of the personality. Dr. Bendit looks to the future of parapsychology for techniques leading to the control and integration of the function in harmonious personality growth.

If Dr. Bendit's title were altered to read "One Theory of Paranormal Cognition and Its Use in Psychotherapy," it would fit his presentation more accurately and would provide the experimentalist with a clearer view of what he has done. What Dr. Bendit has tried is to apply knowledge about parapsychology to psychotherapy. If he has not relied very much upon experimental findings, it is because no matter how profound the implications of the experiments have been for general psychology, very little of it has been directed toward study of individual differences. For example, laboratory experiments suggest that approximately one person in five can demonstrate extrasensory perception. If this holds true generally, and if the ability is normally distributed, then there must be many people whose cognitive life is very different from that of the average person, because of frequent extrasensory perception. In our culture, in which few people really believe in paranormal phenomena, such a difference could be recognized only as abnormality, an abnormality unlikely to be sympathetically understood and very likely to provide the basis for gross personality maladjustment.

Dr. Bendit's main proposals remain matters for experimental confirmation or rejection. Paranormal cognition, or that part of it limited to ESP, may be found most frequently among hysterical and oversensitive people. It may be continuous in process with sensory cognition. It is probably indistinguishable in experience from apperception, imagination, and fantasy. It may be a process rooted in deeply instinctual drives, operating mostly on an unconscious level. But the challenge to the experimenter is clear. If ESP is of human importance, it is reasonable to suppose that it contributes in some orderly manner to the experience of the indi-

vidual. That contribution must be *personal*—directly in the service of the needs, drives, and goals of the person. Laboratory test procedures have necessarily been concerned with the identification and measurement of ESP. The broad psychological question of *why* it should occur has been largely left to speculation and clinical observations. Yet here is a practically untouched experimental field of great potentiality for insights into the nature of ESP.

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LETTERS AND COMMENTS

THE TEXTURE OF THOUGHT, WITH SPECIAL REFERENCE TO TELEPATHY[1]

Dear Rhine:

Thank you very much for your letter and for asking me to write to you. I am at present on holiday and am going to snatch the opportunity of writing about a subject that is basal not only to telepathy but also to many other subjects—the texture of human thought.

I have acquired the habit of suddenly turning round on myself, as it were, and saying, "How did you get that thought? Did you see it? Did you hear it? Did you see the words printed? When a picture is running in your mind, to what extent can and do you control it? How coloured is it?" I have approached dozens of my brother-officers at sea—mind speaks to mind rather intimately on the bridge in the middle watch—passengers and patients, boys in camp, nurses, professors, and my own family. Some of them shy when they understand my question as though one were probing a dark secret, and very few understand it until it has been explained at length. That, I think, is an amazing thing, that so few of these, among whom are some well-educated people, have ever considered in what currency are their thoughts. An Eton classics Master was seriously upset by the effort of introspection that it involves and had to give it up, and when I asked a man who lectures in psychiatry at a London hospital, he mumbled. When I explained my question more fully—When you talk of past events, do you see those past events? When you lecture on the cerebral cortex, do you see it, or do you see or hear the words? Do you ever smell it or feel its soft flabbiness?—he stammered. He said, "Well, words are short-cuts—er. . . ." It became quite obvious that he had never considered the question.

If I may, I am going to give you a summary of my conclusions

[1] The writer of this letter, an English physician, is the author of the article entitled "Missing, Believed . . ." which was published in our September number. We are publishing this letter because we believe it contains many points of interest to our readers, and suggestions for investigators.—Ed.

because it seems to me that this subject is fundamental to the understanding of telepathy and every other subject involving human thought.

If telepathy exists, if thought by some mechanism or other is transferred from one person to another, it surely must be in the same currency. If A is thinking of an umbrella, for instance, as a written word, and B sees a moving picture of it opening, while C's main reaction to the word is a waterproofy-smell-thought, and D, a child, thinks of it in tactile terms as a silky, rustling thing, vaguely but intimately connected with Sunday feelings and Olympian parades to a hollow, echoing, strangely-smelling place called Church, then it seems to be improbable that any transference of thought on this subject is possible between them.

But before I give a summary of my conclusions there are two dogmatic statements I must make because it took me quite a long time before I realized them; and unless an investigator is warned, he is going to waste a lot of time and possibly be misled.

1. Very few people have given any attention to this subject, and when it is explained to them it is necessary to say, "I don't want your answers now; give your attention to the subject from time to time and tell me in a month or in six months." Quick answers, the colostrum, should be thrown aside.

2. This is an amazing fact! A person who thinks mainly in one currency finds it almost impossible to realise not only that other people think in other currencies, but that they themselves used to do the same. Here are two examples from literature—and there seems to be surprisingly little of it on so vital a subject, though I have not overburdened myself with searching, preferring to approach the subject like a field naturalist who has not the temperament to study nomenclatures, books, and museums. Galton, one of the first investigators, found even Royal Academicians, men who painted first-class pictures, who scoffed at the idea that anybody could think in pictures and who painted by a mixture of copying and technique. The other example comes from Rossett's *Mechanism of Thought:* "The thought of the appearance of a person or landscape is devoid of definite outlines or colours." Obviously the author was a word or abstract thinker.

One has simply got to accept these different textures of thought

as a fact—not like the British sailor who said, "The Dago's a poor ignorant blighter. Why, 'e calls an 'at a sombrero!"

Well, to take the most familiar type first, the ordinary, educated or semi-educated middle-aged person like myself. If his thought for a week could be analysed, I think it would come out something like this:

50 per cent in pictures
30 per cent in heard words
10 per cent in abstract thinking
7 per cent in tactile, taste, and smell thoughts
3 per cent in seen words

As a rule, my pictures are coloured, but less so than in real life. The camera is generally my eyes, but sometimes it is further away, because at times I see my own person. I am under the impression that it gets further away as one gets older.

Of course, the various forms of thinking are used for different purposes, which reminds one of the recent physiological findings that the cortical representation of sensation in mammals is in direct proportion to the importance of function. If one is thinking back, going over a scene of yesterday or years ago, pictures predominate, unless the scene has been gone over so frequently that it has been condensed into an abstract thought. If, on the other hand, one is thinking of an interview that is going to take place tomorrow, the heard word will predominate with the picture moving obscurely, half-coloured in the background.

I have met many people, generally people whose lives are spent in clamour of talk, people whose environment is extremely social, who think almost exclusively in heard words. That is to say, they carry on mental conversation with themselves or with shadowy or imaginary persons, and this is their chief currency of thought. In other words, the realities and happenings which pictures represent to the eye are thought of by some people as verbal accounts of the same. Personally I notice my heard-word thinking increases during my busy times in practice and persists for some days after, while my picture thinking comes back as the predominant form when I am alone here in my cottage in Sussex.

Stoddart, in his *Mind and Its Disorders,* also found that the majority of people were visuals and says that he himself was changing from a picture to a heard-word thinker, obviously a change in

response to function as he was head of a large mental hospital with a consultant practice. That is to say, his mental pictures which represented reality were being turned for motives of economy and conciseness and conveyance into heard words.

Let me stress that I don't suppose that anyone is a pure picture, heard-word, or seen-word thinker; the phrases simply indicate the predominant form in the jumble of thought.

Of course there are many other ways of thinking, and some are found only in specialists. I once hung over the side of a ship deep in conversation with a banker, a dried-up, cigar-smoked man, and he told me that he thought in figures and that the memory of the bubbles that swirled past below us would be to him one of countless figures and denominations that he had been unable to docket. The Pacific Ocean was to him a recurring and radiating decimal.

Then thousands of people think very largely in music or in the symbols of their trade.

* * * * * *

As I say, this is a summary and an over-simplification. Human thought is such a jumble of heard and written words, pictures, and symbols (seen, associated, remembered or imagined), taste, smell, and tactile thoughts that if one considers it—and very few do—it is obvious that to write an accurate account of what goes through the mind in five minutes, the texture of thought, would take several days. That, of course, is one of the functions of words, to act as captions, to summarise, to select, sometimes to disguise, but above all to reduce all the methods of thought to a common currency. One has only to live with a solitary person such as the Australian bushman with whom I lived for some months to realise that instead of being currency, words can soon regress to a clumsy form of barter.

And now we come to what would be for me, if it were not for Axiom 2, almost incredible—the seen-word thinker. When I was a student in my finals I did retain in my mind certain pictures of words, lists of drugs and doses, and so on. It was always an effort and just a temporary convenience. Nowadays I use this method only for things like railway connections: "Rye, 8:58; London, 10:45," I see printed on the time-table. Like so many people I can read a novel and get the picture and hardly notice the words. That's what makes it almost incredible to me that there are people who

think only in seen words and that even more condensed form of shorthand, abstract thinking. This is what I wrote about them in a little essay:

> There are great prizes for these visual-word thinkers, these expert signallers in this civilised life of ours, for they are the gold medallists, the men with the amazing memories, the deft debaters, and they are safe men because they are shackled to earth by words and are not lured away by pretty pictures. On the other hand they lose some of the joys of life and I think they may be even a source of national danger because of their limitations; for on account of their qualities they are our leaders. McDougall seems to have thought this, and I fancy Solomon hinted at it in Proverbs 29:18. From people's writing one can often spot what form of thinkers they are—the telegrammic, grudging, schoolboyish style of the picture thinker and the smooth flow of the heard-word thinker are characteristic. I confess I find it difficult to read and understand the seen-word thinker's output, for instance most philosophy and a lot of psychology.

That is one of the great paradoxes of human existence: they who can write books have lost visions, and they who have visions are inarticulate. Often we have to rely on the accident of men like Keats, Stevenson, Wells, Maugham, who having acquired a proficiency in words are violently wrenched back to the visionary world by the action of a disease toxin.

With the seen-word thinker I am classing the abstract thinker and this requires a definition. An abstract thinker is one who has made the shorthand of words shorter still; he thinks of words in terms of another word; of whole sentences which could be analysed down to the realities of eye, touch, taste, and smell in one word; of volumes which contain thousands of these in a condensed phrase or two; and the word for this super-shorthand, this sweeping Olympian condensation, is "concepts," an almighty et cetera! It is the most specialised form of human thinking and adapted to function in the bookish, scholarly, newspaper, examination world; that is, educated civilised life. In a way it may be a degeneration, a form of specialisation from which as in so many specialisations in the biological world there is no turning back. I should not be at all surprised to hear that a liability to it is inherited in Mendelian ratio. I think for telepathic purposes such people can be disregarded. I suggest that anyone who has a large percentage of abstract, seen-word, or heard-word thinking, all of which stand for a form of

communication which obviously supersedes telepathy, can be wiped off as a telepathic subject.

When you say in *Extra-Sensory Perception* that the most successful of your students had a certain artistry or that one or both parents had had psychical experiences or were artistic—I haven't got the book with me and am quoting from memory—I suggest that all that this means is that these young people were picture thinkers. When McDougall said that he himself failed in his experiments with you, it seems plain to me why it was. McDougall, as one can tell from his very readable books, was largely a heard-word thinker, and I think you have got to look for your telepathic ability below this level of specialisation. So far, then, my suggestions amount to this: that you are not likely to get telepathic results from anyone, the bulk of whose thinking is in heard words, still less are you likely to get it from the scholarly seen-word and abstract thinkers. You are much more likely to get good telepathists from the picture thinkers' level and below it. If you can get two people thinking in the same currency or currencies you will be more likely to demonstrate telepathy than otherwise. You can find out the currency or currencies in an individual by getting him to consider the question for some months, or in a more rough and ready way by a questionary such as is found in Titchener's *Experimental Psychology.*

When thought is transferred from one person to another, in what form is it? As a picture, a word, in terms of smell, touch, taste or of that general awareness which we called the sixth sense? As I said before I think it is improbable that it is in words for the reason that words are a form of communication that has been developed for mouth-to-mouth and later written transmission. I think that your card-demonstrated telepathy was done in pictures, but I should expect the other forms of thinking are more easily transferred by telepathy, though they might be harder to demonstrate. Still, a series of different sounds, a series of different tastes or smells might easily take the place of your five emblems on cards.

All the evidence that I have sifted, all the evidence of the psychoanalysts, seems to show that thought is rarely if ever purposeless. There is always some biological need which thought is helping. That is why I very much doubt whether your vestigial telepathy will

ever be much more unless there is a considerable biological urge over a considerable period of time.

An experiment I should like to try would be something like this: A young man going on an expedition or somewhere outside the range of postal service should keep two diaries, and somebody of whom he is very fond, a lover, a sib, a parent, or better still, somebody about whom he is very curious, some form of rival, should do the same. In one diary the writer should record his own doings, in the other what at odd times during the day he has seen the other doing or feeling, when rather in the "free association" method of the psychologists he has thought of him or her. It mustn't be a letter because that brings in the speech element which I surmise has ousted telepathy. On comparison of the diaries one would expect the evidence of telepathy to get stronger as time went by, because the adaptability of a mental function to a need does take time. One would have to see that they were both of the same ideational type and not more specialised than predominantly picture thinkers.

I think it ought to be borne in mind that the sensation or emotion that gives rise to telepathic thought may not always be love and anxiety and tenderness, for from a biological point of view it is just as important to know what the rival or enemy is doing and thinking, so that jealousy and fear and, above all, curiosity may also operate. Nor do I think that the possibility of telepathy between man and beast should be forgotten. As a field naturalist I know that the more tranquil I am the more I see, the closer I get, the more I find, but that may have another explanation.

* * * * * *

What about a child's thought processes, from which all adult forms originate, from which they must grow to perform their various functions?

An adult talking of childhood is inclined to lump those rapidly changing years together rather in the same way as he looks on all the millennia B.C., and for purposes of this letter I too must generalise. A large part of a child's earliest thoughts are tactile, gustatory, and olfactory; and even when he ascends to the stage where there is a large component of visual imagery, there are striking differences between its caricature and an adult's visual imagery. It

is more vivid and can become almost hallucinatory; to the adult eye it is over-coloured. This accounts for what seems to the adult an element of caricature in child thought, the adult having levelled colour, size, and disproportion by use. Words, besides being useful as shorthand and for conciseness, check the improbabilities and wildnesses of picture thinking. (It is significant that the language of the olfactory and tactile is meagre and predominantly hostile.) Only late in childhood does the word check begin to operate; a stool can be turned into a throne or an elephant without this being made ridiculous by being put into words. In a book of my childhood called (I think) *Sir Toady Lion,* a small boy spent the whole morning happily ascending from the nursery floor on to a stool, then on to a chair, turning round and coming down again with a crooked arm. The book said he was pretending that he was going up the steps of the club, having a drink, and coming down again arm in arm with a friend. Cold humiliating words! They don't bring out the glorious feeling of going up steps alone, the taste amounting almost to sensation of vividly coloured bubbling drinks—seen from about two inches—and the freedom to have as many as one likes, the fascinating feeling of long trousers, and the stateliness of a top-hat, the feeling of perfect friendship and contact with an equal, a person of one's own world, indeed of one's own creation, a composite of the most desirable people one knew or had imagined, someone who could be admired, trusted, followed, or bossed as mood demanded. (Extraordinarily like some adults' conception of God!)

The point I want to make about these very ordinary examples of child imagery is this: In the guise of amusement they were fitting the child according to his conception of it for the adult life for which he is destined. That is to say, the function of thought here is preparatory; just as Stoddart became more of a heard-word thinker and less of a picture thinker, so here too thought is functioning as an adapter to environment.

Sometimes this mechanism acts in reverse. Child memories, child thought with its vivid imagery, with its tactile, olfactory and gustatory content comes streaming back as a salve for adult catastrophes. The senile memories of childhood may be in this category, the brutal murderer who called for his mother on the scaffold cer-

tainly was, but I have a more convincing one in my own experience. I once got "bushed" in Australia, and although I knew my compass direction back to my solitary camp I was none the less completely lost in a maze of gullies running between six-hundred-foot cliffs. Except for the little specks of sugar off gum trees I was without food for five days, but had ample water. When I knew I was lost I got into a considerable panic in which I saw the nameless cluster of my whitening bones glaring in the high sun, but later I became most extraordinarily placid. Although the panic returned in lessening waves, the succeeding tranquility prevailed each time. The tranquility was due to the type of my thought. I found myself remembering my childhood not only in incredible detail—individual days—but how I felt with the whole orchestra of my six senses, the greatest of which is the sixth sense—or really it ought to be the first—that chorus from lungs, intestines, joints, heart, which is so clamant in childhood towards brothers, sisters, parents, rooms, corners of the garden; I recalled some of my absorbing inner phantasies which were all of the highly coloured pictorial type, strongly supported by the other senses, a few words thrown in like captions to a cinema film, to hold it at a particularly glorious spot or start it off on a new exciting excursion. It was as though an adult of thirty-five returned to child life (which as Freud says adults completely forget and which as I say they must forget if they are to adapt the texture of their thought to their jobs) and came back and gave some account of that forgotten land. Of course, psychoanalysts make their patients go back like this to childhood, but I want to keep off orthodox psychology, for it has well-worn word tracks which would mislead us. I have never recaptured the Australian bush mood, but then I've never starved for five days again, nor—and this is much more important—have I had the tranquility which was the swing-over from the fear of a lingering lonely death.

My dear Rhine, don't think I am being discursive. I want to pick up this tag-end later on in my final correlation. The tag-end is that not only is thought an automatic trainer, adapter, and salve, but it also counters emotion by calling up or being called up by countervailing emotion.

To return to child thoughts: An adult is apt to give a predominantly pictorial account of child thought because that is the cur-

rency that the child and the adult have in common; but even then the adult often fails to remember the myopic quality of a child's visions. And still more forgotten is the thought in terms of touch and taste and smell and of general awareness coming from other parts of the body at rest or in motion (I am avoiding terms like kinaesthesia because they put us into the long trousers of the seen-word and abstract thinker), but these are the common currency of childhood. A man may be to him a tobacco-smelling piece of rough serge and vaguely hostile, or a woman a rustling piece of silk smelling of lavender water and pyorrhea, probably friendly as it is twice associated with the taste of chocolate, whereas an adult would see their outlines or their names printed.

The reason why I have brought you half-blind to grope and smell your way about this toddlers' world is that here is the basis of human thought, and an understanding of this is most necessary for your search. Somewhere down in these levels you may be able to demonstrate telepathy and clairvoyance in a more perfected form, not as a single-factor unemotional unit, not among them who in recapitulation are living in the post-puberty world of easy speech, still less in those who have caught up with their times and live in a word world and have adapted the texture of their thought to it.

* * * * *

What I want to do now is to suggest two types of drugs or poisons which have the effect of bringing a specialised texture of thought back to a less specialised, of making the adult think more like a child.

The first group is of disease toxins and the ones I am most familiar with personally are tubercle and influenza. I used to find when I had active tuberculosis that it stimulated my picture thinking enormously. The temperature in this disease, as you know, rises about three or four in the afternoon and comes down about 2:00 A.M. with a profuse sweat. I used to look forward to the rise of temperature because then I embarked on a glorious adventure story which I could direct and vary to some extent, a highly coloured, egocentric, blood-thirsty film. Not only was the picture thinking vivid, but tactile, taste, smell, and "feeling"—that is, awareness from the rest of the body, parts not supplied by the special senses—all these classes of thought thronged back. Much of my dreaming

was about the sea, and I tasted and smelt and felt it and heard its myriad voices calling. When one considers tubercle in this light one sees why there are so many tubercular writers. They are men and women who, having acquired the heard-word, seen-word, or abstract-thinker status, are every now and then sent back to the picture-thinking world.

To sum up: Tubercular toxin is an ataviser of the mind; a drug which in its interaction with the body is going to reduce these strange specialisations of the mind to a more natural level—"natural" meaning less civilised or perhaps less urbanised—to a more childlike level.

Influenza gives me coloured pictures of a less happy and more incisive type. Tubercle's stories are serials, influenza's short stories. Two at least of my published short stories owe their origin to the latter. Of course, one has to rob them of their childish and egocentric qualities which make the average dreamer reticent about them before putting them on paper.

The sanatorium, then, does offer a possible field for your investigations. Then, too, enquiry might be made in those cases where telepathic communication seems to have taken place at the moment of death or in severe illnesses. What were those illnesses? Or where the recipient was ill, what was his illness? Acting on the assumption that toxic illnesses bring the texture of thought down to a less specialised level, in a telepathic couple, in a proved case, was the one who was ill, before his illness, a more specialised thinker? And is there any significant difference in telepathic ability in the sexes? Women on the whole are much less specialised thinkers.

With regard to drugs I found that morphia, which I have had about six times, generally for haemoptysis, gives me the most wonderful coloured picture thinking of the child variety, with the bizarre and rather cruel quality that is characteristic of the child. I need hardly say that one would have to be most fearfully careful with any experiments with alkaloids of the pleasant type. Drug addiction is the most terrible thing—I think I'd rather have Nazi domination. But with very small doses and if the subject did not know what he was taking, or with some of the less habit-forming derivatives, well, it is worth considering. Because an experiment is dangerous it does not mean it should not be tried, but one would have to be very, very careful. It would be so easy for the excuse

of experimentation to slip into the habit of addiction. And truly, the drug addicts I have seen are in their deprivation symptoms near to hell.

Aspirin, which I have taken very rarely, I thought tended to make my picture thinking less coloured, more black and white.

* * * * *

When you come to ruminate about it, it is obvious that all thought has its origin in feeling, and by feeling I mean not only the messages of the special senses and of the other parts of the body, but of the emotions too, which really are only a sort of spectrum of special senses. If you take a rapid survey from the time when the first living bit of protoplasm had connection with its watery surroundings and the intimacy of chemical change took place—the first tactile sensation—to the time millions of years later when this simple mechanism had been elaborated to taste, smell, hearing, and sight; letting your mind halt for a moment on that epoch-making event when repeated stimulus brought flagging response, and later when the stimulus was repeated, an altered response because some biproduct of tiredness had laid the first bricks of what was to become human memory, it is borne on one with the conviction of simplicity that thought is elaborated sensation, which is none the less sensation because it is stored.

Thought is never continuous. It comes in waves. And this, too, I have noticed: the more expert the thinker, the shorter the waves, giving one the impression, unless one analyses it, of continuity.

Now my next point is most important and I have been leading up to it for a long time: Not only does a particular kind of sensation determine the line of thought, but the thought has a definite function in relation to that sensation. It is not chaotic and casual as one was inclined to think. The whole body thinks, and it is the function of the brain to coordinate and interpret their calls for help, suggestions, peremptory demands, and subtle reminders, and if necessary draw on that store of old sensations called the memory. After each succeeding wave of panic in the Australian bush my mind came with stored memories of the most secure time of my existence to quell and calm; with adroit and humorous amorality it joined up with morphine to turn a bad half hour into a merry play; with

bombed Londoners it turned what must have been a series of nervous assaults into triumph over fear, exaltations.

Probably only recently in the history of man, since he began to be civilised, has the process been reversed to any extent; that is to say, has thought been an end in itself; master in its own right, unformed by sensation, uninfluenced by that rainbow of sensations called the emotions, independent of those inherited peculiar responses named the instincts; godlike, detached or pretending to be.

Even then it is only for very short periods; the vast bulk of all thinking is in response to sensation and a large part of sensation is in response to thinking, a give-and-take process adapted for a common end.

What relevancy, then, has all this to telepathy, the phenomenon which you, with such infinite patience, have demonstrated in single-factor, pictorial form—in my view, in vestigial form? Just this: If you define telepathy as the transference of thought from one person to another without the agency of any of the six senses (the rest of the body apart from the areas supplied by the five senses is worthy of being classed as at least one sense; it is more likely sixty and they bristle with afferent impulses, especially just before puberty); and if you regard thought as an efferent impulse from these sensations, then two momentous questions arise. The first is: What is the particular sensation that gives rise to this thought? And the second: By what mechanism can the afferent impulses, the appropriate sensations in one person evoke—no, that's not the word —couple on to the efferent thought in another person?

Let's take the first question: What is the sensation? Loneliness giving rise to longing, some crisis giving rise to fear and panic? Surely not, for these would be only occasional events, certainly not continuous enough to give rise to a specialised form of mechanism. It must have been a prolonged and everyday necessity extending over millennia and even then it is possible that its inefficiency drove men into closer association, into communities, into cities.

I imagine this phase of man's existence was something like this: In each family there was in each member an awareness of the whereabouts and doings of each other member at all times of the day and night. A sort of eager curiosity was the vital urge. Remember, it was a sparsely populated world, and each member

of the family was proportionately more important. That would account for the apparent triviality of much telepathic communication and would show perhaps that you were fortunate in selecting meaningless symbols on cards for your experiments. Then to know, say, that a brother had found berries, that the fire that a sister tended had gone out, that the father was uneasy because he had heard a curlew's distant alarm, that a child played contentedly with a frog or had found something that belonged to you, to know the thought of that member of the tribe whose picture thinking was most accurate, whose sixty senses like converging lines of observations that fix a position at sea make his pictures so accurate that they run ahead of time, to know the thought of this clairvoyant member—all this sort of knowledge coming in day and night would in aggregate have a very definite survival value in the nomadic life. In modern life neighbors' knowledge of each other, only partly confirmed by ear and nose and eye will sometimes give vestiges of it. One seems to sense this sort of general awareness in watching grazing deer.

And then the second question: By what mechanism can the afferent sensation of one person *milk* the efferent thought of another? I am not competent to speculate on this, but I guess that the electroencephalograph which is being used in the diagnosis of epilepsy and cerebral tumor—or its successors—will one day give us an answer.

Two more small points: First, in your experiments your students are sitting down in civilised conditions. Theoretically—and I am prepared to bet on it—their scoring should increase if they were walking about in woods and bringing into play another set of senses from moving joints and muscles and wandering eye. Second, telepathy occurs only in an emotional field, probably only a mildly emotional field, which might be termed eager curiosity (I think you called it happy contentment); but even this is apt to disappear in the cold arid presence of the seen-word and abstract thinker.

Oh, and finally, in order to get your subjects in a proper emotional state of eager curiosity, they should be subjected beforehand to a prolonged course of considerable boredom.

You are on a fascinating trail. It may lead you, as I think it will, doubling back to an interesting phase in man's development,

an old abandoned form of communication like post-boys; or—and this I fear—up some arid sandstone valley where the echoes of your figures and emotionless tables come mocking back to you; or—and this we all hope—to expanding horizons, (for to us mortals there is no finality), to some peak in Darien.

Yours ever,
C. G. L.

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference. Words defined elsewhere in the glossary are italicized in the text of the definitions.

*For a simple description of those terms marked by an asterisk, as they apply to the ESP test data, see Chapter VIII and the Appendix of *A Handbook for Testing Extra-Sensory Perception* by C. E. Stuart and J. G. Pratt. A mimeographed copy of the relevant pages will be sent on request to subscribers who do not have the book mentioned. Further explanation may be found in any elementary statistical text.

AGENT: In tests for *telepathy*, the person whose mental states are to be apprehended by the *percipient*. In *GESP* tests, the person who looks at the *stimulus object*.

AVERAGE SCORE: Average number of *hits* per *run*.

BM (BLIND MATCHING): The technique in which the *subject matches* a *deck* of *ESP cards* to five *key cards* which are laid out face-down before him in an unknown order. Unless otherwise stated, the order is also unknown to the experimenter.

BT (BEFORE TOUCHING): The technique in which the top card of the face-down *deck* is *called* and, after being called, is laid aside for *checking* at the end of the *run*. Each card in the *deck* is treated in the same way.

CALL v.: To attempt to identify a *target* or *stimulus object* (or mental state of an *agent* in *telepathy*).

CALL n.: The *response* described above; also the resulting selection.

CHANCE:* The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely *score* if only *chance* obtains.

CHANCE AVERAGE: *Mean chance expectation* in terms of *average* per *run*.

CHECK: To determine a *score* after the completion of a *run* by comparing the order of the *subject's calls* with the order of cards in the deck.

CHI-SQUARE: A sum of quantities each of which is a *deviation* squared divided by an expected value. Also a sum of the squares of *CR's*.

(Occasionally the square of a simple CR may be used as chi-square.)

CLAIRVOYANCE: *Extrasensory perception* of objective events as distinguished from *telepathic* perception (of the mental or subjective events of another person).

COVARIATION: Correlation evaluated in terms of theoretical means and *standard deviations.*

CR (CRITICAL RATIO):* A measure to determine whether or not the observed *deviation* is *significantly* greater than the expected random fluctuation about the *average.* The CR is obtained by dividing the observed *deviation* by the *standard deviation.* (The *probability* of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the *score averages* of two samples of data divided by the *standard deviation of the difference.* (Where the samples to be compared are of equal number of runs, the difference between total *hits* may be divided by the *SD* of the total number of *runs* of both samples.)

DECK: Twenty-five *ESP cards,* five of each suit.

DEVIATION:* The amount an observed number of *hits* or an *average score* varies from the *mean chance expectation* or *chance average.* A *deviation* may be total (for a series of *runs*) or average (per *run*).

DIE THROW: The throwing or mechanical release of a single die regardless of the number thrown at the same time.

DT (DOWN THROUGH): The technique in which the cards are *called* down through the *deck* before any are removed or *checked.*

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of *ESP.*

ESP (EXTRASENSORY PERCEPTION): Response to an external event (perception) not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

ESP TESTS: A considerable number of techniques come under this heading which are conveniently represented by initials, the principal ones being: *BT, DT, PT, GESP, BM, OM, STM.*

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to *chance* alone.

FREE MATERIAL: *Stimulus objects* that are not limited to a known number of categories.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of *extrasensory perception,* permitting either *telepathy* or *clairvoyance* or both to operate.

HIGH-DICE TESTS: Tests of *PK* in which the aim of the *subject* is to try to influence a pair of dice to fall with the two upper faces totaling eight or above.

HIT: The correct correspondence of a *subject's call* or response with a *stimulus card* or *object.*

HIT FREQUENCY DISTRIBUTION: The grouping of the total *hits* in a *series* of *runs* with respect to their original position in the *run.*

KEY CARD: One of the five cards (where there are five suits) against which the cards of the test *deck* (i.e., *target cards*) in the *matching* tests (*OM, BM, STM,* etc.) are *matched.*

LOW-DICE TESTS: Tests of *PK* in which the aim of the *subject* is to try to influence a pair of dice to fall with the two upper faces totaling six or below.

MATCHING: A form of *calling* in which *a target card* is placed opposite the *key card* which the *subject* selects to identify it. Also, in the evaluation of *free material,* the act of a judge in identifying a given *response* with a *stimulus object.*

MEAN CHANCE EXPECTATION; see CHANCE.

OM (OPEN MATCHING): The technique in which a *subject matches* a *deck* of *ESP cards* to five *key cards* which are face-up before him.

P (PROBABILITY):* A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with the paranormal—those psychical effects which appear not to fall within the scope of what is at present normal and recognized law.

PERCIPIENT: The person who makes the *calls* in a test situation.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a *subject* without any known intermediate energy or instrumentation.

RESPONSE: The act of the *subject* in attempting to identify the *stimulus object.*

RSR (RUN SALIENCE RATIO): A measure of *salience* within the *run.*

RUN: A succession of *trials,* usually the *calling* of a *deck* of 25 *ESP cards* or symbols. In *PK tests,* 24 single *die throws* regardless of the number of dice thrown at the same time.

SALIENCE: The relation of rate of success in the end *segments* of the *run* to that of the middle *segments;* also the relation of the rate of success in the end *trials* of the *segment* to that of the middle *trials.* TERMINAL SALIENCE: A higher rate of *deviation* in the end *segments* of the *run* (or in the end *trials* of the *segment*) than in the middle *segments* (or *trials*).

MIDDLE SALIENCE: A higher rate of *deviation* in the middle *segments* of the *run* (or in the middle *trials* of the *segment*) than in the end *segments* (or *trials*).

SCORE: The number of hits made in one *run.*
TOTAL SCORE: *Score* of any number of *runs.*
AVERAGE SCORE: *Total score* divided by number of *runs.*

SCREEN: An opaque barrier used between the *subject* and the card or *agent.* The main types of screens are illustrated in this JOURNAL on their first introduction in print.

SD (STANDARD DEVIATION):* The theoretical root mean square of the *deviations.* It is obtained from the formula $\sqrt{npq}$, in which n is the number of single *trials,* p the *probability* of success per *trial,* and q the *probability* of failure. (For *ESP cards,* $SD = 2\sqrt{\text{no. of } runs.}$)
SD OF THE DIFFERENCE: For both *ESP cards* and *PK tests* using dice, the *SD* of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the *SD* of a single *run* and R_1 and R_2 are the number of *runs* in the respective samples compared. This gives the *SD* of the difference for *run score averages.*

SEGMENT: One of the five consecutive sets of five *calls* in a *run* of 25 *trials.* The first five *calls* would constitute the first *segment;* the second five, the second, etc.

SERIES: Several *runs* that are grouped in accordance with a stated principle.

SEVENS TESTS: Tests of *PK* in which the aim of the *subject* is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE:* A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. Common criteria are: a probability value of .01 or less, or a *deviation* in the expected direction such that the *critical ratio* is 2.33 or greater.

SINGLES TESTS: Tests of *PK* in which the aim of the *subject* is to try to influence dice to fall with a specified face up.

SR (SALIENCE RATIO): A measure of the relation of the rate of success in the end *segments* of the *run* (or in the end *trials* of the *segment*) and that of the middle *segments* (or *trials*). (For details of the manner of obtaining SR's, see Vol. 5, pp. 193-195.)

SSR (SEGMENTAL SALIENCE RATIO): A measure of *salience* within the *segments* of the *run.*

STIMULUS OBJECT: The *ESP card* or drawing or other object, some identifying characteristic of which is to be apprehended by the *subject.*

STM (SCREENED TOUCH MATCHING): The technique in which the *subject* makes his *call* by pointing to one of five positions or exposed *symbols* under a special *screen*. The experimenter places the *target card* so designated in the position pointed to. The *screen* blocks all vision by the *subject* of the *cards* and their manipulation by the experimenter.

SUBJECT: The person who is experimented upon. Most commonly the *percipient* in *ESP*, though also the *agent* in *telepathy*.

TARGET: In *ESP tests*, the *stimulus object*. In *PK tests*, the faces of the die (or combination of faces) which the *subject* attempts to bring up in the act of throwing.

TARGET CARD: The *card* which the *percipient* is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The *deck* of cards the order of which the *subject* is attempting to identify.

TARGET FACE: The face on the die which the *subject* tries to turn up as a consequence of direct mental action.

TELEPATHY: *Extrasensory perception* of the mental activities of another person. It does not include the *clairvoyant* perception of objective events.

TRIAL: A single attempt to identify a *stimulus object*.